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ORIGINAL ARTICLES.

I.—The Rôle of Solution in Valley-making.

By A. J. Jukes-Browne, B.A., F.G.S.

(WITH A PAGE MAP.)

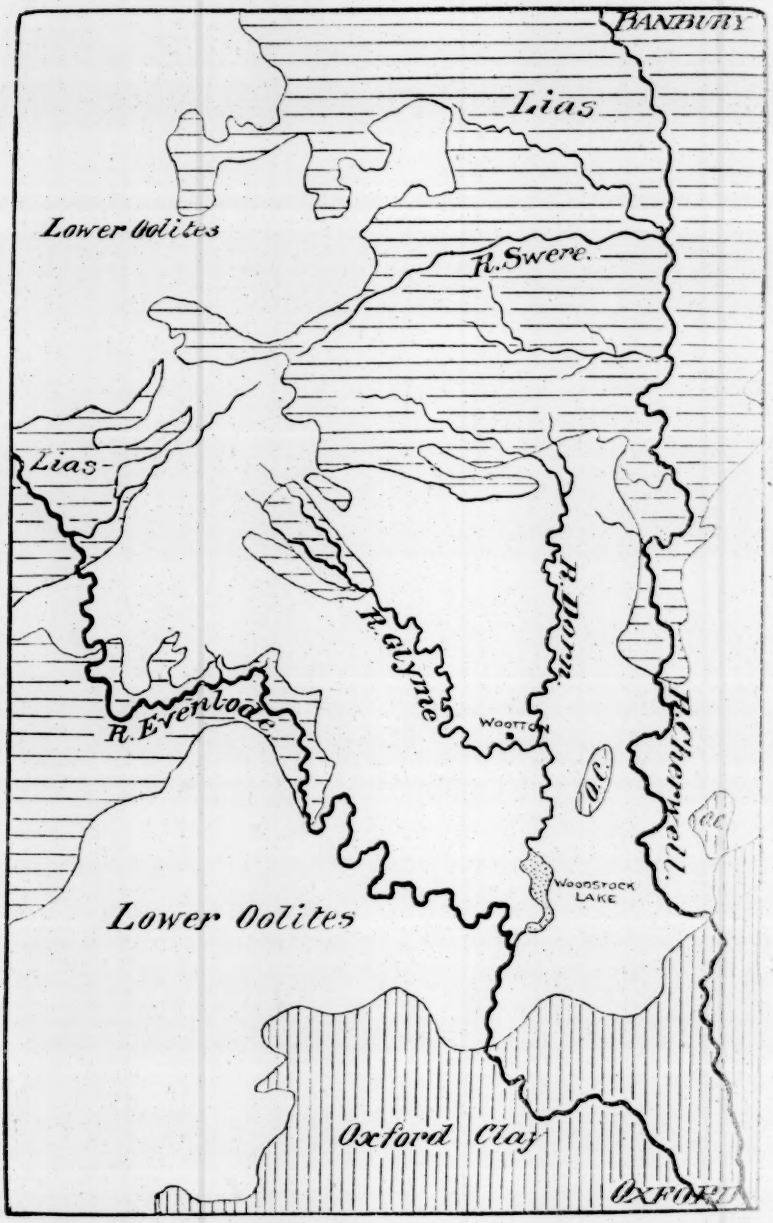
IT has recently been suggested that many of the dry valleys which are found in limestone districts owe their formation to subterranean solution, and not to surface-erosion; in other words, that they are the work of subterranean watercourses which, by dissolving the rock substance, have caused subsidence of the surfaces overlying the lines of such watercourses.

This theory has been put forward as a complete explanation of a whole system of valleys to the entire exclusion of the current view that the chief agent in the excavation of all valleys is the mechanical action of surface water-flow. The new theory assumes that in certain districts there were no surface streams, that their drainage was entirely subterranean, and that the existing valleys have been formed by the gradual subsidence of the surface above the courses of the underground streams.

It has always been acknowledged that solution plays an important part in the enlargement and deepening of the valleys in limestone districts, but the idea of such solution has been chiefly connected with the chemical action of the surface stream-water, and not with the action of such water as sinks beneath the river bottom. It may perhaps be admitted that we needed a reminder that a dry valley may also be deepened by solution, but I very much doubt if the solution is carried on in the manner imagined by the Rev. E. C. Spicer.[1] I think he pushes his theory much too far, and that he credits underground waters with much more concentrated power in the way of solution and valley-making than can possibly be attributed to them.

We are asked to believe that a definite system of underground streams can be formed, without any surface-system, in such districts as the Great Oolite plateau of Oxfordshire and the Chalk area of the Chiltern Hills. We are told that the valley-system of the Glyme and

[1] See "Solution Valleys in the Glyme Area (Oxfordshire)," by the Rev. E. C. Spicer, M.A., F.G.S.: Quart. Journ. Geol. Soc., 1908, vol. lxiv, pt. 3, pp. 335–44, pls. xxxviii, xxxix.



SKETCH-MAP OF PART OF OXFORDSHIRE, ON THE APPROXIMATE SCALE OF 3 MILES TO THE INCH.

[Reproduced by permission of the Rev. E. C. Spicer, M.A., F.G.S., from p. 336 of his paper "Solution Valleys in the Glyme Area (Oxfordshire)," Quart. Journ. Geol. Soc., 1908, vol. lxiv, pp. 335–44.]

Evenlode has been determined by a double set of joints: "percolating water," he says, "will reach a master-joint or a series of master-joints and dissolve out a winding course underground. The water from the neighbouring joints will gradually tend to leak into the master-joint line, and thus a winding area of weakness will be established which tends continually to widen. The ground above this weakened line will slowly subside, and at length the weakened material will be entirely removed, exposing the stream" (op. cit., p. 341).

This proposition assumes that the rocks of the district are traversed by a double set of joints which coincide approximately with the windings of the streams, but no evidence is adduced to prove that this is actually the case, and his own map shows that the windings of the Glyme and the Evenlode are so irregular that they cannot be reduced to the intersection of two sets of lines. This map is reproduced on the opposite page, so that the reader can judge for himself.

Again, it is assumed that a fairly equal amount of subsidence will take place along the whole course of the supposed underground stream, the result being that a surface-valley is produced which exhibits such an extraordinary resemblance to a graded valley of erosion that no one has hitherto suspected it to be anything else. I should have expected the subsidence to have been very unequal.

Mr. Spicer appears to have been afflicted with ghosts, and to have suffered from some form of *erosion-nightmare* as badly as Sir Henry Howorth suffered from his 'Glacial nightmare,' for the former writes, evidently with a sigh of relief, "the spectre of erosion which appears to haunt almost every valley upon the earth's surface must, I think, be banished from the Great Oolite plateau." If Mr. Spicer will examine his 'spectre' a little more closely I think he will find it to be a very real and effective power, and not by any means a mere deceptive phantom.

Mr. Spicer thinks that "the valleys under consideration have no single confirmatory mark of erosion," and that "there is no sign of the past existence of any surrounding heights sufficiently great to have produced streams strong enough to carve out the valleys by mechanical erosion. The whole region appears to have been never anything but a gently-tilted plateau."

Does he, therefore, imagine that the rain falling on the surface of this plateau has never gathered into surface-streams, even during the great Glacial era of frost, snow, and rain, and that running water has never accomplished any erosion in this particular district? Does he imagine that this plateau was the same in Miocene and Pliocene times as it is now, that it never extended further west, and that the Oxford Clay never spread over the area through which the Glyme and the Dorn now run? Has he not completely ignored the significant testimony of that outlier of Oxford Clay near Tackley, which he has nevertheless been careful to insert on his map; and the other outliers of the same clay to the west of Evenlode, which are omitted from his map?

Three of the omitted outliers occur in the area where the words "Lower Oolites" are engraved on the map, and there is another large one to the west of Woodstock Lake. These outlying tracts enable us

to look back to the time when the present basset-surface of the Lower Oolites was entirely covered by Oxford Clay, and when the outcrop of the underlying limestones lay further north across the Banbury district; doubtless also forming higher ground than the plain of Oxford Clay. Such must certainly have been one phase in the gradual development of scarp and valley during Tertiary time.

Surely it is more reasonable to regard these courses of the Glyme and the Evenlode as valleys which originated on a clay-cover in the usual manner, and have been incised into the underlying limestones. I feel sure that in this new theory the cart has been put before the horse, and that the whole valley-system of the Oolitic plateau was formed in the ordinary way by the mechanical action of rain and running water long before the plateau was reduced to its present condition; but there can be little doubt that some of the features which the valleys now exhibit are attributable to the chemical action of the water which runs into the waterways and sinks underground.

In brief, my conviction is that the valleys have swallowed the ancient watercourses, and that it was the latter which determined the courses of the subterranean streams, and not any hypothetical system of joint-planes. It is quite possible that under present conditions more material may be annually removed by chemical solution than by mechanical erosion, but I do not think that solution would be specially active along the valleys unless they had been previously formed by surface-erosion. On this view it is easy to understand why there is only one valley-system, but if Mr. Spicer's theory were correct we ought to find traces of an ancient system of mechanically-formed valleys which did not coincide with the subsequently-formed 'solution-valleys.'

In another paper [1] Mr. Spicer has applied his theory to the valleys of the Chiltern Hills, and Mr. F. J. Bennett has independently evolved a similar theory in explanation of certain interrupted valleys which traverse the Hythe Beds of Kent.[2]

Mr. Spicer remarks that "the strongly marked Chiltern valleys run in various directions, but they are in the main joint-valleys. There are no heights sufficient to produce them by superficial denudation, there are no alluvial fans showing the results of underground action . . . They are too varied in trend and too marked in character to be due to any possible cause that can be logically suggested, except one, but they display universally the characters and results that would naturally arise from the percolation of acidulated water through rock so easily soluble . . ."

This seems to me an extraordinary series of statements. How can valleys which run in various directions be "in the main joint-valleys"? In my opinion their arrangement is that of a natural system of surface rainfall drainage, and Mr. Spicer quite ignores what I have written about the former extent of a clay-cover (i.e. the Clay-with-flints) all over this region. Why he should expect to find 'alluvial fans' I do not understand, nor why (if present) they can

[1] Geographical Magazine for September, 1908, p. 288.
[2] Op. cit., p. 277.

show underground action. As regards the 'marked character' of these Chiltern valleys, I suppose he means their depth and the manner in which they have been cut back into the escarpment-ridge, and sometimes even through it. I cannot see anything in these characters incompatible with the view that these valleys are the modified relics of an ancient system of drainage superimposed upon and finally transferred to the Chalk.

The high-level gaps in the escarpment are surely the truncated portions of valleys which date from a time when its frontal face lay much further west. The deeply-sunk terminations of the valleys inside the escarpment-ridge are due partly to the solution of the chalk along the previously established waterways, and partly to the frequency of landslips when the rainfall was greater than it is at present.

In this connection I would observe that the part taken by landslips in the formation of these valleys has not been sufficiently considered, and I must plead guilty to having omitted any mention of landslips or of solution when describing the formation of valleys in Chalk districts in a recent memoir,[1] though they were duly credited with a share of the work in my paper on the 'Clay-with-flints.'[2]

I believe that small landslips are more frequent in the dry valleys of Chalk districts than is commonly supposed. I have seen many such landslips on slopes of solid chalk where there were no permanent springs and no impervious substratum. The valleys I have in mind are the upper parts of those on the dip-slopes of the Chalk escarpment in Wilts and Dorset; these have very steep sides, the angle of incline being often between 25° and 28°, and small slips from these slopes often take place after heavy rains, leaving bare faces of chalk with a ruckle of soil, rubble, and turf below, the whole forming a conspicuous scar on the prevailing green turf slope. These slips seldom exceed 5 or 6 yards in width, but they illustrate one of the processes which have operated both in widening the valley and in causing the recession of the valley-head. If such slips occur at the present day, how much more frequent must they have been in earlier Pleistocene time, when the precipitation of rain and snow was so much greater than it is now?

Chemical solution must also have been operative in deepening these valleys, but I think it acts from above downward and not from below upward. After a heavy rainfall a part of the water falling on the escarpment and on the ridges between the valleys is directed into the valley-ways, where it soon sinks beneath the surface, and having acquired some carbonic acid and some humic acids in its descent of the valley-slopes, it cannot fail to exercise a solvent action on the chalk below the valley-floor, and thus to deepen the valley by the removal of chalk in solution.

But this solution process is confined to the depth of a few feet from the surface, and is not the process imagined by Mr. Spicer. He expressly says that the subterranean water-flow "will always tend to produce a master-joint, which will carry an underground stream in

[1] "The Cretaceous Rocks of Britain": Mem. Geol. Survey, 1904, vol. iii, p. 418.
[2] Quart. Journ. Geol. Soc., 1906, vol. lxii, p. 158.

hard limestone, but in chalk will tend to produce by solution a constantly widening and deepening area of weakness, above which the superincumbent strata will sag and produce at length a deeply sunk valley."

In the discussion on the papers read at the Geographical Society Dr. A. Strahan mentioned that "in Dorset where dry valleys are dissected by the cliffs it may be seen that the underlying chalk is rotten and undergoing solution." This is perfectly true, but the observation does not support Mr. Spicer's theory, as Dr. Strahan seemed to suppose. The chalk under such valley-floors is decomposed and disintegrated into a loose mass of chalky paste and rounded pellets of chalk, which is evidently due to the solvent action of water percolating down from the surface; whereas on Mr. Spicer's solution-theory the cliff-section of such a valley ought to show an underground channel or fissure widened by the passage of water, and all the chalk above this widened fissure should show signs of the disruption, sagging, and subsidence which he postulates.

The facts and arguments brought forward by Mr. Spicer and Mr. Bennett afford ample proof that chemical solution by percolating water has played an important part in the deepening of certain valleys, but I cannot see any good basis for their view that these valleys were initiated by solution, nor for Mr. Bennett's theory of the "upward hydrostatic action of underground water." It should be mentioned, however, that Mr. Bennett fully admits the former existence of clay-covers and the co-operation of mechanical erosion in the process of valley-making, so that his view is much more rational and scientific than that of Mr. Spicer.

To sum up, therefore, I think that the actual initiation of valleys in Chalk districts must be ascribed to the influence of surface conditions which have long since ceased to exist; and that the special features which they now present can be explained by the action of surface-causes which are still visibly in operation, namely, landslips and solution of the valley-floors. I cannot see that there is any difficulty in accounting for the dry valleys of limestone districts, or any reason for inventing a special theory of 'solution-valleys' to account for them.

I do not know the area described by Mr. Bennett, and consequently I cannot discuss the origin and formation of its valleys, but I should have thought that the phenomena which he describes were as easily explained on the old theory of the valleys having swallowed the surface-streams as on his new theory of the swallow-holes having initiated the valleys.

II.—On the Occurrence and Origin of Laterite and Bauxite in the Vogelsberg.

By J. R. Kilroe, A.R.C.S.I., H.M. Geological Survey of Ireland.

A RECENT visit to Central Germany afforded the writer an opportunity of studying the conditions under which the Vogelsberg iron-ore and bauxite have been formed, and a brief account of what was observed may assist in broadening the basis upon

which a sufficiently comprehensive view of the origin of those substances may ultimately stand. On this account my visit to the region was approved by the Department of Agriculture in Ireland, in connection with the work now in progress on the iron-ore and bauxites of co. Antrim. In my traverses I enjoyed the very agreeable companionship of Dr. Schottler, of the Grossherzoglich Hessischen Geologischen Landestaldt, and the advantage of his guidance. His elaborate description of the basalts in the vicinity of Giessen [1] has since been issued, and is referred to in this paper. The admirable and exhaustive accounts of the bauxite by Professor Adolf Liebrich,[2] and of the iron-ore by H. Münster,[3] are also appealed to frequently. Among other important papers consulted regarding the chemical processes involved may be mentioned those of M. H. Coquand,[4] Professor G. A. Cole, F.G.S., M.R.I.A.,[5] Max Bauer,[6] Sir T. H. Holland, A.R.C.S., F.R.S.,[7] H. Warth, D.Sc.,[8] & F. J. Warth, B.Sc.,[8] Malcolm Maclaren, B.Sc., F.G.S.,[9] H. Brantwood Muff, B.A.,[10] and the summaries by F. Wigglesworth Clarke [11] and Dr. C. Doelter.[12]

The principal points which came under my observation may be briefly summarized as follows :—

(*a*) In the Vogelsberg the ores do not lie in a common zone, or in a zone or zones geologically distinct, and are not associated with lignite.

(*b*) Laterite with iron-ore is developed in irregular tracts on each side of a post-basalt dislocation.

(*c*) The best iron-ore (limonite and non-pisolitic earthy ore) has accumulated below a thick more or less ferruginous layer of completely weathered basalt, clay with pseudo-stratified arrangement.

(*d*) This ore is formed chiefly, if not wholly, from lavas of the lowest stage, while the bauxite is chiefly derived from those of the succeeding stage.

(*e*) The ore-bearing clays carry only a thin covering, if any, of Post-Pliocene date, and are of superficial origin.

(*f*) The date of their origin, therefore, probably late Pliocene.

Taking these points *seriatim* we find—

[1] Abhandlung der Grossherz. Hessisch. Geol. Landes. zu Darmstadt, Band iv, Heft iii, Wm. Schottler.

[2] 28th Bericht der Oberhessischen Gesell. für Natur- und Heilkunde.

[3] "Die Brauneisenerzlagerstätten des Seen- und Ohmtals am Nordrand des Vogelsgebirges": Zeitsch. für Prak. Geol., 1905, p. 242.

[4] "Sur les Bauxites de la Chaine des Alp.": Bull. Soc. Géol. de France, tome xxviii (1871), p. 111.

[5] "The Rhyolites of the County of Antrim": Sci. Trans. Roy. Dub. Soc., 1896, ser. II, vol. vi.

[6] "Beiträge zur Geologie der Seyschellen": Neues Jahr. für Min. Geol. und Palæontologie, 1898, p. 163.

[7] "On the Constitution of Laterite": Geol. Mag., 1903, Vol. X, p. 59.

[8] "The Composition of Indian Laterite": Geol. Mag., 1903, p. 154.

[9] "On the Origin of certain Laterites": Geol. Mag., 1906, Dec. V, Vol. III, p. 536.

[10] "East Africa Protectorate."

[11] "Data of Geochemistry": Bulletin No. 330, U.S. Geol. Survey, 1908.

[12] "Petrogenesis," 1906.

(*a*) The volcanic series near Giessen is divisible into two groups according to Schottler,[1] viz. :

α. Basische Basalte, with 40·6 to 46·7 per cent. of silica, subdivided into basalts with porphyritic and granular structures respectively. The lavas of this stage attain a thickness of about 20 metres.

β. Trappgesteine (anamesites), containing 45·4 to 53·8 per cent. of silica, and reaching some 30 to 40 metres in thickness, perhaps more.

γ. A third group is added—jüngere basische Strombasalte—to be seen near Kloster Arnsburg, etc., referred to by Münster[2] after Schottler.

These members of the series are well defined and traceable in the field with precision. Yet at no point has it been found that the junctions are marked by a definite zone of either bauxite or laterite. In my traverses I passed from the Lower Basalt to the Traps, east of Watzenborn, where the rock is well exposed, without perceiving indications of any such zone. Near Kloster Arnsburg also, where the Upper Basalt rests on the Traps, in the river bank of the Wetter, only a little bole, tuff, and a very thin seam of white indurated clay marked the junction of the two groups.

(*b*) The area is traversed by several post-basalt faults, dislocating the members of this otherwise undisturbed series. One of these, an important 'fossa,' runs southward by Mücka, along the Ohm- and Seen Valley, on each side of which, as shown by Münster,[3] laterites (including ferruginous and aluminous clays with iron-ore and some bauxite) have accumulated to a considerable depth, and to irregular distances up to some 2,500 metres from the fault-line. This observer has shown that the peculiar alteration of the basalt has proceeded from the boundary plains of the component masses—pieces of breccia, exfoliating balls, etc.—towards the centre of each mass, in some cases leaving cores of undecomposed basalt; that the Brauneisenerz —'Stückerz' of the miners—accumulates in the boundary joints, and is removed from the adjoining clay by washing; and that a considerable accumulation of this ore exists beneath the yellow, red, and brown lateritic clay, and in the basalt, some 16 to 18 metres from the surface.

The clay is studded here and there with exfoliating lumps of aluminous material, grey and yellow coats alternating around central cores of undecomposed basalt, precisely comparable to those seen in the Antrim lithomarge. Sketches which I have made of these roundish masses in the Grube 'Hoffnung,' near Stockhausen, might serve as illustrations of those to be seen at the Giant's Causeway,[4] and near Parkmore and Carnlough in Antrim.

(*c*) The yellow and red clay resulting from the disintegration of the basalt is often strongly aluminous, and, as at rothen Hang, to be afterwards referred to, contains lumps of bauxite with basalt structure. At Grube 'Luse,' east of Mücka, these lumps are so numerous as to

[1] Abhandlung der Grossherz. Hessisch. Geol. Landes. zu Darmstadt, Band iv, Heft iii, pp. 331, 451–67.

[2] Op. cit., p. 244. [3] p. 243.

[4] See paper by Tate & Holden, Quart. Journ. Geol. Soc., 1870, p. 155.

form a conglomerate over the iron-ore layer, as mentioned by Münster.[1] The bauxite occurs in comparatively insignificant quantity in the Mücka region; the chief mining industry there is that of iron-ore, which is for the most part dug out of open pits. A good section is to be observed at the Grube 'Hoffnung,' where at the time of my visit a large systematically worked pit had reached the approximate dimensions of 150 by 200 yards and 40 to 50 feet in depth. The section seen was somewhat as follows:—

	Feet.
'Diluvial' matter with stones of basalt	0 to 2
Other superficial material, chiefly sandy clay, termed Löss by Münster	5 to 8
Yellow and grey clay with dendritic manganese oxide	3
Yellow, grey, red, and brown clay with 'Stückerz,' exfoliating basalt lumps, and nests of pyrolusite	25 to 30
Layer of weathering basalt	12
Limonite (Stückerz or Braunerz), sometimes concretionary and stalactitic, with clay	6

(*d*) The basalt from which the laterite just described has been formed is believed by Dr. Schottler to be entirely that of his first stage, though Münster thought it consists, in part, of the 'Trap' or Anamesite. The difference is immaterial, but the point is mentioned because when we pass to the chief area of bauxite, this ore, and the aluminous clay containing it, at rothen Hang, east of Garbenteich, are derived entirely from the Trappgesteine of Schottler.

The road from Lieh, westward to Garbenteich, leaves a somewhat elevated area on the right, known as hohen Stein, opposite which on the south side stretches undulating ground covered with red clay containing pieces of bauxite, which may be picked out of the soil. Westward of hohen Stein the road runs between Auf der Haide and rothen Hang, on both of which the bauxitic clay also forms the soil. That on rothen Hang is the chief source of bauxite, and the area covered with the peculiar clay amounts in the aggregate to about a square kilometre; it reaches a depth or thickness of about 1½ to 2 metres. The layer has all the appearance of having been formed in place by peculiar weathering and disintegration of the anamesite, to be seen *in situ* throughout the elevated tract of hohen Stein.

Describing the bauxite, Liebrich writes as follows:—"The bauxite of the rothen Hang shows unmistakeably its origin . . . the yellowish grey bauxite showed clearly a medium-ground anamesite."[2] And again: "The bauxite of this region is very nearly related in structure to the anamesite of hohen Stein. The small pieces of bauxite which occur at the foot of the hohen Stein, in the weathered products of the anamesite, have exactly the same structure as this. The size and number of the transformed plagioclases correspond completely."[3] Pieces of bauxite up to half a metre in diameter have been met with in pits, and, as Liebrich notes, in no case has a core of unweathered basalt been found in the bauxite lumps. Notwithstanding

[1] "Die Brauneisenerzlagerstätten des Seen- und Ohmtals am Nordrand des Vogelsgebirges": Zeitsch. für Prak. Geol., 1905, p. 251. [2] Op. cit., p. 72.

[3] p. 75. Some beautiful points are indicated by Liebrich in this connection, viz., that the basalt-ironstone pieces, also met with in the bauxitic clay, have the same structure as the anamesite, and that the proportions of Fe_2O_3 and Al_2O_3 in the ironstone, and Al_2O_3 and Fe_2O_3 in the bauxite, correspond atomically—they alternate.

this, there can be little doubt that the clay and contained ore are the result of peculiar weathering in place which the basalt has locally undergone.

(*e*) It is a very striking fact that the ores and containing clays, both at Mücka and east of Garbenteich, have little or no superficial covering. In the latter place I was unable to notice any. To sustain some comparison with the conditions in Antrim I hoped to find the red clay, etc., at rothen Hang passing, in indurated form, under the columnar anamesites of hohen Stein; but Dr. Schottler assured me that in a well-boring put down 30 or 40 metres at the Abdeckerei des Kreises, near the Lich-Garbenteich road, nothing of the kind was met with. The clay with bauxite on the rothen Hang is therefore absolutely superficial, and the laterites at Mücka, when not so, bear no covering earlier than the Loess.

(*f*) The volcanic history of the Western Vogelsberg commences with tuffs in the Giessen Sands, which correspond to those at Münzenburg, to the south-east, and at Marburg on the north. These follow the Oligocene of Mainz, and are therefore believed to be of Miocene age. The interbedding of tuffs, basalt, and sands ceases at Giessen before the stage of the 'Traps' is reached, so that the series as a whole is considerably younger than the basalts, etc., of Antrim.

Moreover, the region, as stated previously, is traversed by post-basalt faults, i.e. later than the third stage of volcanic outpourings: and it must have undergone considerable denudation since the faulting, to produce the present surface features. Münzenburg Hill, which is a neck from which lava streamed forth, is now isolated from the group. The pretty feature of Schiffenberg retains only a thin cap of glassy Trap, while it overlooks sedimentary strata which must have been perhaps thickly covered with both Traps and some Lower Basalt. The Frankfurt $\frac{A}{M}$ Section 18, of the geologische Karte by Lepsius (1 : 150,000), shows Pliocene strata in some of the Vogelsberg valleys, e.g. those of Allendorf and Zell and one near Weitershain. It seems quite just, therefore, to infer that the present contours are of Pliocene age, and that the surfaces, in relation to which the ores have been developed, are the result of late Pliocene sculpturing; and even if we take the palms of the Münzenburg Sandstone as indicative of a warm climate, we must come much nearer to a Post-Pliocene date and glacial conditions, for the origination of the Vogelsberg laterite and bauxite, than the Miocene or Eocene, with the climatic conditions which then prevailed in our latitude.[1]

Origin of Laterite and Bauxite in the Vogelsberg.

It is noteworthy that these substances originate for the most part under tropical conditions.[2] These conditions are stated as essential by

[1] It interests me to find that Münster questions whether a tropical climate has been a factor in the formation of the Vogelsberg iron-ores and bauxites. Op. cit., p. 257.

[2] Whether in Ireland or in Southern France in earlier geological times; or in India, the Deccan, Malabar, etc., to-day; Georgia, Alabama, Arkansas; Brazil, Surinam; the Congo, East Africa; the Seychelles, Java, Sumatra, and the Hawaiian Islands—all have their aluminous clays and ores: and so natural has it become to assume a hot climate that Bauer assumes it (Neues Jahrbuch, 1898, p. 219), a view mentioned without comment by Doelter ("Petrogenesis," 1906, p. 235).

Maclaren: "Lateritic deposits require for their formation (*a*) tropical heat and rain with concomitant abundant vegetation; (*b*) alternating wet and dry seasons."[1] Holland, too, assumed the necessity for tropical conditions when relying upon the intervention of bacteria for the production of laterite. Whatever value may be attached to this suggestion, it is freely admissible that the chemical processes involved, organically, in the abundant growth and rapid decay of vegetation, or in the inorganic reactions proceeding in the earth's crust, are greatly stimulated by heat; but are tropical or sub-tropical conditions requisite? The laterite (red clay) described by Muff occurs at such an elevation[2] in East Africa that heat could scarcely be claimed as an essential condition for the peculiar alterations in question; he holds that the decay of forest vegetation—acids resulting therefrom—is a chief factor in the transformations.[3]

The usual impressions regarding the necessity for tropical or sub-tropical heat were in one's mind in going to Germany; with this, however, was my conviction that atmospheric carbonic acid, carried into the earth by rain, played a more important part than the one usually assigned to it if acting under favourable geographical and climatal conditions. I have had to modify my convictions regarding the necessity for heat, for the Vogelsberg laterites, etc., were formed at the threshold of the Glacial Period, as has been shown above, and my belief that CO_2 played a very important part has been strengthened. Abundant forest growths often accompany deposits of laterites and bauxites, the decay of which would have given off quantities of this gas. But where no obvious proof of former vegetation exists, as, in my experience, is the case in Hesse, CO_2 issuing from fissures as a post-volcanic product, as suggested by the carbonated springs at Nauheim and at several

[1] Geol. Mag., 1906, p. 546.

[2] Some 9,500 feet. Op. cit., p. 42. If tropical heat were the determining condition, should we not expect lateritic deposits everywhere throughout the tropics, declining in importance northward and southward in the temperate zones, wherever basalts, dolerites, etc., appear at the surface? Münster, while questioning the existence of tropical conditions during the formation of the laterites at Mücka, resorts to thermal springs, with Chelius and Delkeskamp (op. cit., p. 257), as the effective cause. The carbonated waters of the springs, he believed, brought up iron and bore it into the laterite layer, as well as promoting the weathering of the basalt. As to this I would remark—

(1) The laterite layer, always superficial (practically), occurs from 1 to 3 kilometres from the fault along which the supposed springs issued. Could the waters have retained heat to such distances and have affected only a superficial layer of rock?

(2) The peculiar weathering has proceeded from above downward, the greatest concentration of iron-oxide being at the bottom. Is this not analogous to the case of iron-pans in soils on a larger scale?

(3) The amount of iron given up by the basalt, in weathering to clay in the uppermost 20 feet, could easily account for the aggregate of limonite deposited in the lower 20 feet.

(4) May not the CO_2 given off by springs, such as those now at Nauheim and along other lines of post-basalt dislocations, including that at Mücka, have been carried into the earth from the atmosphere, and affect the rock more generally than when issuing only at points here and there?

[3] G. H. Kinahan relied upon the leaching action of the organic acids from decomposing peat to account for the formation of bauxite. Trans. Manch. Geol. Soc., 1894, vol. xxii, p. 458. This is mentioned by Clarke, op. cit., p. 420.

points in the Vogelsberg region, may formerly have been so copious as to increase largely the proportion of this gas in the lower strata of the atmosphere, when still, over the tracts where bauxite and laterite are now found. Dr. Schottler believes that post-basalt faults traverse the ground north and south of Watzenborn, i.e. each side of the rothen Hang bauxite tract.

E. Kaiser, as well as Münster, rely upon carbonated waters to have effected transformations of the German basalts,[1] and Maclaren assumes, and in part discusses, the reactions of CO_2 in water in the solution of silicates.[2] In a preceding footnote I raise the question whether spring water, either as a warm solvent or one acidified with CO_2, can penetrate throughout a *superficial* layer of rock or clay, as far as would be necessary to suppose it effective; and if this were doubtful in a layer 50 feet deep, in proximity to the fault at Mücka, it would seem much more doubtful east of Garbenteich, where the layer is little more than a tenth of that depth. The circumstances of distribution of the ores in both places strongly suggest that, if CO_2 were the solvent, it must have been borne to the earth from an atmosphere heavily charged with it, given off by fissure springs such as those previously mentioned.

It has naturally been questioned whether CO_2 can be regarded as an adequate solvent, and some observers have appealed to the action of stronger acids. J. Walther, and especially Passarge,[3] appealed to nitric acid supplied by decaying rich tropical vegetation, and formed during thunderstorms; but this, in quantity at least, would seem very inadequate. Others have favoured H_2SO_4 and H_2SO_3.[4] Such acids, formed from pyrites (say) in the Vogelsberg 'Traps,' could not affect the alumina, for in the weathered layer the amount of SO_3 would be immeasurably smaller than is requisite, even for the solution of Mg and Ca, as may be seen by the following analyses. A is an average of ten analyses of *Trappgesteine* after Schottler, B and C are analyses of bauxite and bauxitic clay given by Liebrich (p. 71):—

	A.	B.	C.
SiO_2 ...	50·34 ...	1·10 ...	26·84
TeO_2 ...	2·06 ...	3·20 ...	
Al_2O_3 ...	13·11 ...	50·92 ...	53·89
Fe_2O_3 ...	4·53 ...	15·72 ...	20·21
FeO ...	6·25		
MgO ...	8·34 ...	·16 ...	·23
CaO ...	8·07 ...	·80 ...	·42
Na_2O ...	3·16		
K_2O ...	·96		
P_2O_5 ...	·49		
SO_3 ...	·066		
CO_2 ...	·16		
OH_2 ...	2·24 ...	28·60 ...	18·50

[1] Clarke, "Data of Geochemistry," p. 424.

[2] Particularly it present in such strength as to produce intensive action (Geol. Mag., 1906, pp. 539–40).

[3] "Petrogenesis," p. 235.

[4] Amongst the first to propose this was Professor G. A. Cole; see "The Rhyolites of the County of Antrim," p. 108. Clarke mentions the emanations from volcanoes, as in Java, Sumatra, and Hawaii, op. cit., p. 425. See also G. C. du Bois and Bauer, quoted by Doelter, "Petrogenesis," p. 235; and Hayes and Liebrich, referred to by Clarke, p. 421.

The sulphuric acid brought up with CO_2 in springs, as that at Carlsbad,[1] could only be effective very locally, and, when we turn to the consideration of sulphurous acid vapours, we cannot overlook the following points, viz.:—

1. The deleterious, if not death-dealing, effect of such vapours on vegetation; workable seams of lignite sometimes exist side by side in the same zone, and of contemporary formation, with a rich seam of iron-ore over a deep and wide-spreading lithomarge.

2. Nothing is more striking than the even way in which solution and deposition of Al_2O_3, as hydrate, must have balanced each other in the aluminous layers of rothen Hang. Solution and deposition must have been all but simultaneous; more correctly, perhaps, they rapidly alternated. In following the process, if we regard the solvent as H_2SO_4, and the precipitant $CaCO_3$ in solution, then, although Al_2O_3 could have been precipitated freely by $CaCO_3$ in lake waters, as is suggested by Coquand for the occurrence of bauxite in Bouches du Rhone, it is extremely difficult to imagine how rapid alternations of the process could have gone on in close, deep rock interstices where we assume ordinary percolation and capillary action. For when $Al_2K_2(SO_4)_4$ in solution below, meets $CaCO_3$ in solution, descending, the interstices would become choked with precipitated $CaSO_4$ and $Al_2O_3\,3OH_2$ to impede, if not entirely hinder, both percolation and capillarity.

Strong acids were no doubt engaged in the processes, chiefly in the initial stages, after which the chief solvent would have been CO_2. Maclaren maintains that "energetic chemical action in the presence of abundant carbon dioxide" effected complete decomposition in the conditions under which Indian laterites were formed. It is hazardous to propose definite formulæ in attempting to represent what probably takes place, where reactions must obviously be so complex. Many processes are probably at work, simultaneously; and it is very doubtful if the simple formulæ proposed by Maclaren,[2] or that by Van Hise quoted by the Editor of the Geological Magazine, for the sericitization of felspars, or any representing transformations through zeolites[3] or substances allied to them, could—however tempting such formulæ might appear—present all that takes place, or even the main line of reactions along which alteration would run. Keeping in mind the chief desiderata, viz., the detachment and bearing away, in soluble form, of silica in some proportion from the silicates; also the detachment of alumina, and its retention in a nicely balanced condition—now in solution, now precipitated as hydrate—the following general formulæ representing mass-action would seem to include the principal stages in the process of transformation. Let R'_2O be the alkalies; $R''O$ the alkaline earths and ferrous oxide; and R_2O_3 the alumina and ferric oxide; also let a compound silicate be affected in the first instance by H_2SO_4; then $a(R'_2O)$, $b(R''O)$, $c(R_2O_3)$, $d(SiO_2) + n\,H_2SO_4 \rightleftarrows x\,R'_2SO_4 + y\,R''SO_4 + z\,R'_2R_2(SO_4)_4 + X$, where X represents

[1] Watts' Chem. Dict., vol. v, p. 1017.

[2] Geol. Mag., 1906, pp. 539–40.

[3] I have found compact basalts as highly transformed as were those which had been manifestly zeolitic, and under similar circumstances.

the shaken siliceous compound after some of the alkalies, alkaline earths, iron oxide, and alumina have been withdrawn as sulphates. Again,

$$X + z\,R'_2\,R_2\,(S\,O_4)_4 + p\,C\,O_2 + q\,O\,H_2 \rightleftarrows$$
$$r\,Si\,O_2\,(O\,H_2)_2 + s\,(Al_2\,O_3, n\,O\,H_2) + u\,R'_2\,C\,O_3 + v\,R''\,C\,O_3 + Y,$$

where Y represents the still more shattered compound.

Here now we have colloid silica, which will dissolve in an abundance of the newly formed alkaline carbonates, and either entirely disappear or become diffused through the mass perhaps to form secondary quartz; and free alumina, to form perhaps its unstable carbonate, say when the supply of $C\,O_2$ is abundant, and precipitate, when that supply slackens, as well as to go afresh into solution when the supply again increases.

If we suppose $H_2\,S\,O_4$, formed from pyrites in the rock, as suggested by Hayes; or, as suggested by Professor Cole, from post-volcanic vapours—present, say, in such small quantities as not to be injurious to vegetation—this acid would have been likely to initiate the attack upon the silicates. The molecular constitution of these compounds having been thus shaken or disturbed, the remaining substance, made up of constituents in a nascent state, would have been laid open to further attack, and probably wholesale solution as shown above, by $C\,O_2$ in abundance, in rain-water carried down from a heavily charged atmosphere: in this regard, Maclaren suggests, would the conditions differ from those under which common clays are formed, namely, those in which $C\,O_2$ is small in quantity and consequently weak in action.[1] The conditions also differed from those in which kaolinization of granite, etc., takes place, by the action, as H. Rösler shows, of thermal springs and vapours.[2] The alkalies, $Mg\,O$, $Ca\,O$, and $Al_2\,O_3$, it is believed, would all be attacked and in time go into solution; and, wherever the acid became satisfied, the nascent $Si\,O_2$ would become dissolved by the alkaline carbonates—present, we may suppose, in large quantity. Thus would the delicately balanced state be reached in which, while capillarity is maintained in free play, a large quantity of $Si\,O_2$ might be carried off or be deposited at certain points, according as the solution remained alkaline or became acid; and the alumina could be transferred from point to point throughout the weathered mass to concentrate in bauxite above[3] or in the interstices of lithomarge below.

Replying to queries of mine regarding the possibility of accounting for the Vogelsberg bauxite by supposing the former existence of forests in the region, or decaying vegetation yielding humous and carbonic acids, Dr. Schottler has sent me the following note, November 9, 1908, viz.: "Unsere Bauxite liegen vielmehr stets an der Oberfläche; auch sind keine Anzeichen dafür vorhanden, dass er sich unter einer dichten Vegetationsdecke oder unter Waldbestand gebildet hat. Ebenso hat man noch nie versteinertes Holz in dem Ton vom roten Hang gefunden. Verkieselte Hölzer kommen nur als Seltenheit hie und da im Tuff vor."

[1] Geol. Mag., pp. 539–40.

[2] Zeitschr. für Prak. Geol., 1905, p. 333. Paper by O. Stutzer in which he gives, with approval, Rösler's view.

[3] Liebrich, op. cit., p. 96. The author believes that $Al_2\,O_3$ was dissolved out of the rothen Hang clay and concentrated in crevices, etc., of the bauxite lumps, as hydrargillite.

III.—Studies in Edrioasteroidea.[1] III. *Lebetodiscus*, n.g. for *Agelacrinites Dicksoni*, Billings.

By F. A. Bather, M.A., D.Sc., Brit. Mus. (Nat. Hist.).

(Plate XXV.)

Previous History.

THE specimen herein to be considered is one of great historical interest, for it was the first specimen of an Edrioasteroid made known to science. It was discovered by Dr. J. J. Bigsby in limestone now recognised as of Lower Trenton age, forming Table Rock at the Chaudière Falls on the Ottawa River at Ottawa (then called Bytown), Canada, in 1822. Brought by Bigsby to England, it was figured and described, though not named, by G. B. Sowerby in 1825.[2] E. Forbes, who had the specimen for study, referred to it in his memoir "On the Cystideæ of the Silurian Rocks of the British Islands,"[3] since the "aspect" of his *Agelacrinites Buchianus* "immediately called [it] to mind"; he even went so far as to say that there could "be no question . . . of its being generically allied" to that species. Considering the not unnatural inadequacy of Sowerby's description and figure, the reputation that Forbes had as an authority on echinoderms, and the comparative imperfection of the first found specimens of *Edrioaster*, it was not surprising that E. Billings in 1856[4] should have supposed a new Trenton fossil, undoubtedly congeneric with *Agelacrinites Buchianus*, to be of the same species as that found by Bigsby, and should therefore have applied to it the trivial name '*Bigsbyi*,' while giving to a fossil of obviously different structure the name '*Agelacrinites Dicksoni*.'[5] In February, 1858, Billings travelled to London with the fossils in question, and found that Bigsby's specimen was not, after all, the same as his *Cyclaster Bigsbyi*, but was specifically identical with his *A. Dicksoni*. He redescribed the species, and had his type-specimen, as well as Bigsby's fossil, figured by C. R. Bone.[6]

The latter specimen was said by Billings to be then "in the Museum of Practical Geology, Jermyn Street, London." I therefore supposed that it had been transferred to the British Museum when all the foreign fossils were so transferred some years ago. But when no trace of it could be found either in the collections or the registers of that establishment, I applied to Mr. E. T. Newton, palæontologist to the

[1] Studies I and II were published in the Geological Magazine for December, 1898, and May, 1900. Publication of the present Study, written in 1899, was delayed owing to an unwillingness to load Zoology with a new generic name without further confirmation from all available evidence. Since that date so much Edrioasteroid material has passed through my hands that the publication of these Studies is resumed with more confidence.

[2] "Notice of a Fossil belonging to the Class Radiaria, found by Dr. Bigsby in Canada": Zool. Journ., vol. ii, pp. 318–20, pl. xi, fig. 5; London, October, 1825.

[3] Mem. Geol. Surv. Gt. Brit., vol. ii, pt. ii, 1848; see pp. 519 and 520.

[4] Rep. Progress Geol. Surv. Canada, 1853–6, p. 292; Toronto, Autumn of 1857.

[5] Op. cit., p. 294.

[6] Canadian Organic Remains, dec. iii, p. 84, pl. viii, figs. 3 and 3*a* (the holotype), 4 and 4*a* (Bigsby's specimen).

Geological Survey, and he kindly made a search which was at last successful. I have to thank him and the then Director General of the Survey for graciously allowing me to retain this specimen from 1897 till the end of 1898. It bears the following labels:—"Agelacrinites Bigsbii / Falls of the Chaudière / Ottawa Rivr. Canada / Pres. by Dr. Bigsby 1848 / E F △", "M.P.G.," "6259." The label suggests that Forbes really did fully share the original misapprehension of Billings.

In 1881 a figure described as "Specimen of *Agelacrinites Dicksoni* from the Cabinet of Dr. Grant" was published as fig. 9 of a plate illustrating "Description of a new species of Porocrinus, &c.," by James Grant, M.D., etc. (Trans. Ottawa Field-Nat. Club, No. 2, pp. 42–4). No reference to the specimen was made in the paper or elsewhere in the number.

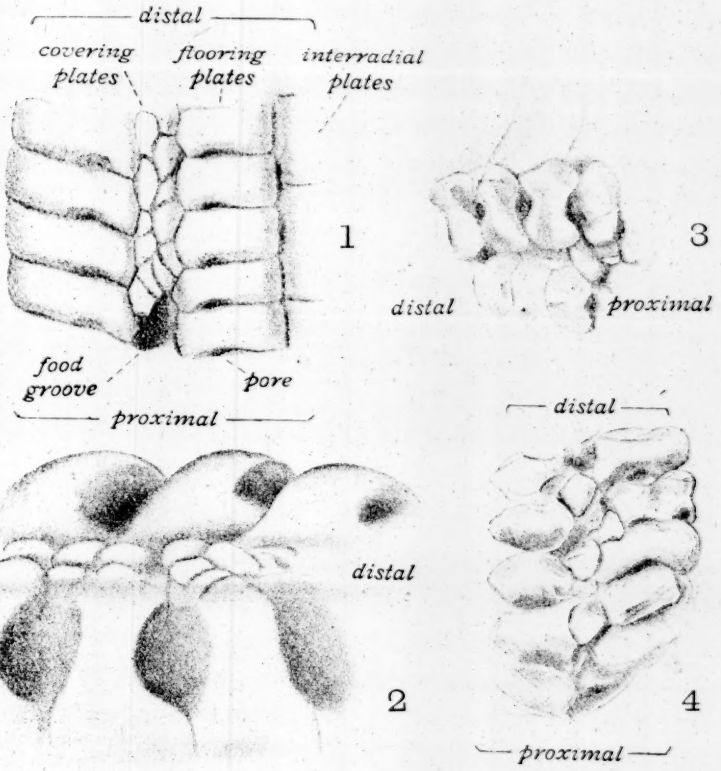
Description of Bigsby's Specimen.

The specimen overhangs the edge of a triangular fragment of limestone, with sides respectively 56, 70, and 76 mm. long. The rock is composed of fragments of coral, monticuliporoids, and pelmatozoa; bits of undeterminable brachiopod shells are visible, but I cannot detect the "single spiral univalve" which Sowerby says "is also to be observed." There is, however, on one side a fragment of some subcylindrical object, with irregular longitudinal striæ on its surface; it is fully 6 mm. long and 2 mm. in diameter. The whole has the characteristic black tint of Trenton Limestone. The upper surface is curiously weathered, and on it lies a fragment, apparently of a *Pleurocystis*, with a part of one of the brachioles. The two echinoderms have the actual test preserved in highly crystalline carbonate of lime coloured by iron rust. The rugose surface of the Edrioasteroid may be due to partial solution and redeposition of calcite; the study, and especially the figuring, of the specimen is thus a task of much difficulty. The under surface of the specimen is obscured by the matrix, even where it overhangs. An attempt to remove some of this has revealed a few doubtful traces of plates. The posterior and right and left posterior interradii are fairly preserved, but the other two are incomplete, and from the left anterior the greater part of the test has been removed.

The periphery is very obtusely pentagonal; as to this I do not share Sowerby's hesitation. The theca rises above the periphery to a height of about 5 mm., which height is attained by the plates of the radial grooves and of the right posterior interradius. The plates in the latter, however, have been raised since death. The above-mentioned traces of plates suggest that the under surface was not flat, and one might hazard a conjecture that the total height of the animal was about 6 mm.; but it may have been excavated around the abactinal pole. The sagittal diameter was about 20·5 mm. The greatest width, measured along the right posterior interradius and left anterior radius, is 23·5 mm. The length of a side is roughly 11·5 mm.

On the Upper Surface the five Subvective Grooves radiate with a sinistral curve, which, to a distance of about 6 mm. from the actinal centre, is hardly perceptible, but then becomes more pronounced.

The grooves reach the periphery, but do not curve round it or pass over it. They would therefore have been invisible from the under surface. The left posterior groove attained a length of 19 mm. from the actinal pole; the left anterior and right posterior were about 1·5 mm. shorter; the others are too imperfect to measure accurately.



SUBVECTIVE SKELETON OF *Lebetodiscus Dicksoni*.

FIG. 1.—Portion of a ray, showing relations of covering-plates to food-groove and flooring-plates; slightly diagrammatised. × 6 diameters.

FIG. 2.—Portion of a ray, seen in three-quarter perspective, showing covering-plates and the excavation of the flooring-plates; slightly diagrammatic. × 20 diam.

FIG. 3.—Portion of the anterior ray, showing how pores are formed by lateral excavation of flooring-plates; sketched under the microscope. × 6 diam.

FIG. 4.—Similar appearances in another part of the same ray, with some covering-plates; sketched under the microscope. × 6 diam.

(All drawings made from Bigsby's specimen.)

The skeleton of the subvective grooves is stout and raised above the general surface to a height of about 1·2 mm. in their proximal regions. The width near their proximal ends is 2·8 mm.; they taper gradually distalwards, and are rounded off rather bluntly. Each is clearly seen

to be composed of a double row of flooring-plates, alternately arranged. These are rounded, and have in places a slight appearance of overlapping by their proximal margins. In the proximal region of a groove, the long axis of any one of these plates is directed almost at a right angle to the median line of the groove, but tends in a distal direction; its length is 1·6 to 2 mm.; the breadth of such a plate is ·9 to 1 mm. There are from 21 to 25 plates along each side of a groove: the lower numbers are on the adjacent sides of the right and left pair. The greatest number is on the adanal side of the left posterior ray.

Each of these plates is excavated on its sides, but more on the side towards the distal end of the ray (Fig. 2), so that the adjacent excavations form a pore, which lies about half-way between the middle line and the outer margin of the whole ray. The actual food-groove was very narrow, and appears to have been covered with irregular plates, some of which are preserved in places (Fig. 1). These covering-plates, though small in proportion to the whole structure, are large compared with the food-groove and do not seem to have had a regular alternating arrangement. As to their existence, I have no doubt; but the state of preservation of the fossil forbids a more accurate account.

The actinal centre is roofed over by relatively large irregular plates, one or two of which are broken away on the right side, so that one gets a suggestion of the underlying hollow, or vestibule of the mouth. These covering-plates appear to be serially homologous with those of the grooves, and their relations to them and to the flooring-plates are best seen in the right posterior ray.

The Interradial Areas are bounded by the flooring-plates of the grooves and by the periphery, but were not separated by any differentiated area from the under surface. They are covered with relatively large and apparently thick plates of irregular shape. The adoral margin of each plate slightly overlaps the adjoining margin of the adjacent plate. This gives the effect of a slight adoral imbrication, more perceptible the nearer the periphery. The plates become a trifle smaller towards the periphery, but there is no sudden diminution of size, and no break in their continuity with the peripheral plates of the under surface. The plates are rugose, but it is hard to determine precisely how far this is due to original ornament. At about half-way between the actinal centre and the periphery the width of the right posterior area is 5·4 mm., that of the posterior area 8·5 mm. The distance from the distal extremity of the left posterior ray to that of the right posterior ray is 15·1 mm., to that of the left anterior is 13·6 mm. The distance between the distal extremities seems to have been a little less than 13·6 mm. in the other interradii, but exact measurements are not obtainable.

The conspicuously greater width of the posterior interradius is due to the presence of the Anal Opening, clearly recognisable as such. Its centre is 6·5 mm. from the actinal centre, and nearly on the middle line of the interradial area. The plates of the valvular pyramid, if they ever existed, have disappeared. The opening which remains is surrounded by a border of plates smaller than the others of the same area, into which they merge, and slightly raised above them, but not

governed by a definite arrangement. The diameter of the opening is about 2 mm.; that of the whole ring is about 4·5 mm.

The Under Surface is so obscured by a hard matrix that one cannot, even after many days labour, be sure of its structure. Fragments seen below the end of the left posterior ray and of the posterior interradius, towards its right side, suggest that there was a pavement of irregular polygonal plates, about 9 to a square millimetre, not imbricating, but probably set in a flexible integument. A broken edge visible in the left anterior interradius, suggests that the larger plates around the periphery formed a stoutish frame, and that the minutely plated central integument stretched loosely across from this frame. Such a suggestion is at least in accordance with the little that we know of this region in the Edrioasteridæ. At the same time there is room for doubt whether all the appearances actually proceed from portions of the individual.

Remarks on the Holotype and on Billings' Description.

The type-specimen of this species is "a fragment, consisting of one perfect ray and two of the interradial spaces," preserved in the Museum of the Geological Survey of Canada, at Ottawa. Since this was the only specimen in the possession of the Survey in 1856, I assume it to be the same as that taken by Billings to London, and figured in Decade iii, pl. viii, figs. 3, 3*a*. These figures, however, show two complete rays and considerable portions of three others. Only two interradial areas, however, are at all complete, and neither contained the anus. No central aperture is to be distinguished. The specimen "is quite flat, and appears to have been firmly attached." There is no reason to doubt that Bigsby's specimen, as well as the "other specimens" alluded to by Billings, were rightly referred by him to this species.[1] Moreover, since his second account was actually based largely on Bigsby's specimen, any differences between that account and the description now given must be differences either of observation or interpretation. Let us consider them.

"The diameter . . . is from three-quarters of an inch to an inch and a half." Billings' fig. 4 represents our specimen as $1\frac{1}{16}$ inch along a diameter which is really $\frac{13}{16}$ inch. It is therefore a medium-sized specimen. The type-specimen, as drawn, would have had a diameter of $\frac{13}{16}$ inch, and is therefore a very small specimen.

"The rays . . . are bounded by two rows of small plates, which . . . arch over the grooves. The upper ends of the plates on one side meet those of the opposite side, in a line along the centre of the ray, thus forming for each ray a sort of covered way." In other words, the plates called by me 'flooring-plates' were regarded by Billings as covering-plates, and he did not see the true covering-plates at all, which he was hardly likely to do unless he specially looked for them. But, besides this, the plates in question do not arch over so as to form a covered way, but occupy the full thickness of the ray, as may be seen in section at the end of the anterior ray of Bigsby's specimen.

[1] Should it ever be proved that Bigsby's specimen is of a different species, it will have to receive a new name; and that new species must then be taken as the genotype of *Lebetodiscus*.

"In all the specimens . . . the rays curve round to the right hand." As the figures show, this means sinistrally or contra-solar, as is actually the case in Bigsby's specimen.

"The marginal plates of the rays do not appear to alternate regularly." They do alternate, however, and the contrary appearance is due to the irregularities of the covering-plates.

"There are two rows of small circular indentations on each side of the rays, corresponding in their position to the ambulacral pores of *E. Bigsbyi*, only that in the latter they are in the bottoms of the grooves." I only see one row of 'indentations,' but the appearance of another row is occasionally produced on the extreme edge of the ray by the rounding of the ends of the flooring-plates. The structure of the anterior ray in our specimen leaves me in no doubt that the indentations were actual pores, corresponding to the pores of *E. Bigsbyi*, which had only one row of pores, not two, as Billings supposed.[1] The difference between the two forms really is that here the pores are well outside the covering-plates, whereas in *Edrioaster* they were roofed in by them, a fact not known to Billings.

Remarks on other Specimens referred to *A. Dicksoni*.

The specimen formerly belonging to Dr. James Grant has already been mentioned. Since the present paper was first written, Dr. J. M. Clarke, in a valuable article entitled "New Agelacrinites" (Bull. N.Y. State Mus., vol. xlix, pp. 182–98, pl. x, December, 1901), has published a diagram based on Grant's figure (p. 190, fig. 3), and from his legend it appears that the diameter of the specimen is only 21·5 mm., and not 48 mm. as it appears in the original drawing. These illustrations indicate some resemblance to Bigsby's specimen, but the small adorally imbricating plates seen on the margin in the right anterior interradius do not appear in harmony with the adjoining plates of the periphery, which are all large, just as they are in Bigsby's specimen. At all events, such a figure, unsubstantiated by any description, cannot be held to prove the existence of the imbricate border characterising *Agelacrinus*, *Lepidodiscus*, and a few other genera.

A distinct imbricate border of *Agelacrinus*-type is shown in pl. ii, fig. 2, of Professor Otto Jaekel's "Stammesgeschichte der Pelmatozoen" (Berlin, 1899). The specimen, which is in the collection of Professor Frech at Breslau, and comes from the Trenton Limestone of Ottawa, is assigned by Dr. Jaekel to *A. Dicksoni*. On p. 50 he says of this species: "Thecalplatten zwischen den Ambulacren besonders gross, Ambulacra ziemlich kurz gedreht, Saumplättchen stark skulpturirt." The word 'Saumplättchen' is usually translated by 'covering-plates' or 'ambulacrals,' and in the explanation to pl. ii Dr. Jaekel applies the word 'Ambulacralia' to two of the plates that in his opinion cover the subvective groove. These are strongly pitted on the sides

[1] See the diagrams and brief account of *Edrioaster Bigsbyi* in "A Treatise on Zoology," ed. E. Ray Lankester, vol. iii, Echinoderma, p. 209. Also F. A. Bather, "What is an Echinoderm?" 1901, and Encycl. Brit. Suppl., Art. Echinodermata, 1902.

(perhaps for the reception of smaller plates), but do not appear to be of the same character as either the flooring-plates or the covering-plates in Bigsby's specimen of *A. Dicksoni*. It is, in fact, clear that the specimen figured by Dr. Jaekel differs in its peripheral zone and in its subvective skeleton from our species, and that in those points it has the character of an ordinary *Agelacrinus*.

With the language used by Dr. Jaekel it is not easy to reconcile the following sentences in Dr. Clarke's paper (op. cit., p. 191):—"Billings claimed that in the Trenton species *A. dicksoni*, perforated ambulacral plates were exposed, but this observation has not been confirmed and Jaekel holds that no ambulacral plates were present in these bodies. At all events usually only the cover plates have been observed." I am not sure what this means, but it is certain that the plates between which Billings described 'indentations' were those here called flooring-plates, and it is highly probable that these indentations were podial pores. It is also certain that these same plates were identified by both Billings and Jaekel with the 'covering-plates' or 'ambulacrals' of a crinoid arm, and that they did not mention the smaller covering-plates, which, in my opinion, are the only homologues of crinoid 'Saumplättchen.'

Of recent years the only other reference to the species has been the record of its occurrence in Trenton Limestone at Pakenham, Ontario, by Dr. H. M. Ami (Ann. Rep. Geol. Surv. Canada, xiv, p. 84 J; January, 1905).

Systematic Relations of the Species.

There still are problems to solve with regard to *Agelacrinites Dicksoni*; but there are problems presented by the majority of Edrioasteroidea, and we certainly know enough to make comparison with other forms profitable.

Taking the families of Edrioasteroidea as defined in Lankester's "Treatise on Zoology" (vol. iii, pp. 207–9, 1900), we may at once set aside the Cyathocystidæ with their massive theca, and the Steganoblastidæ with their stem.

Turning to the Agelacrinidæ, with which the species has always been placed, we see that from *Stromatocystis* it is separated by the imbrication of the interradial plates and the curvature of the rays. The latter feature also distinguishes it from *Cystaster* and *Hemicystis*. It is further separated from *Cystaster* by the large size of its interradials, and from *Hemicystis*, *Agelacrinus*, *Streptaster*, and *Lepidodiscus* by the absence of the differentiated marginal zone, which in those forms is always obvious and often highly differentiated. I also incline to regard it as having had a less flattened and less sessile habit than the genera just mentioned. Similar features, as well as the clear alternation of the flooring-plates of the grooves, enable us to discriminate between it and the little-known *Haplocystis* of Roemer. As for the Carboniferous form to which in 1897 Gregory gave the name *Discocystis*, we know, at all events, that it had no imbricating plates, and that the margin was more distinct than in *A. Dicksoni*.

A more important character than any of those mentioned is presented by the structure of the subvective skeleton. It seems

clear that the side-plates, here called flooring-plates, are homologous with the flooring-plates of *Edrioaster*. Whether those plates have homologues in the Agelacrinidæ is matter for debate; at any rate, no genus of that family has similar plates with intervening depressions so like pores. The covering-plates also seem homologous with the covering-plates of *Edrioaster*, and it is doubtful whether the so-called 'ambulacrals' of the Agelacrinidæ are of the same nature; if they are, they have, at any rate, different relations to the adjoining plates.

Proceeding then to compare the species with other Edrioasteridæ, we note that it differs from them in the restriction of the grooves to the actinal surface, in the small size of the covering-plates, and in the absence or very slight development of an abactinal frame.

The species therefore appears to represent a generic type hitherto unrecognised, and a type of considerable interest in that it is intermediate in so many features between Edrioasteridæ and Agelacrinidæ. What bearing this may have on the classification of the Edrioasteroidea must be reserved for discussion after more of these Studies shall have been published. For the present, the following diagnosis may be offered.

Lebetodiscus[1] gen. nov.

An Edrioasteroid, with theca flattened below, convex above; no marginal zone on actinal surface; no definite abactinal frame; interradial thecal plates relatively large, with slight adoral imbrication; rays curved [contra-solar in genotype], and reaching but not passing the periphery; subvective skeleton of alternating flooring-plates, with intervening pores, and small irregular covering-plates.

Genotype: *Lebetodiscus Dicksoni* (E. Billings, sub *Agelacrinites*).[2] Lower Trenton Limestone, Ottawa.

It may be worth noting that a restored representation of the fossil herein described, by J. S. and A. B. Wyon, adorns the reverse of the Medal founded in 1887 by Dr. Bigsby and awarded biennially by the Council of the Geological Society of London. To judge from the illustration facing p. 252 of Mr. Horace Woodward's "History" of the Society (1907), the medal gives a good general idea of *Lebetodiscus Dicksoni*.

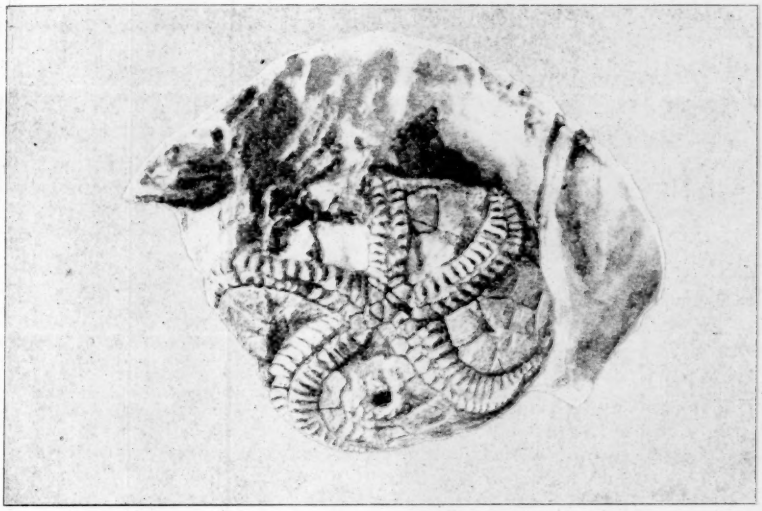
EXPLANATION OF PLATE XXV.

The Upper Figure is taken from a photograph of Bigsby's specimen of *Agelacrinites Dicksoni* Billings, the type of *Lebetodiscus*, enlarged two diameters. Only a part of the matrix is shown. The posterior interradius, with the anus, is towards the observer.

The Lower Figure represents the Lower Trenton Limestone of Table Rock at Chaudière Falls, Ottawa River, where Bigsby collected the specimen figured above.

[1] *Lĕbētodiscus*, from λέβης, a cauldron; after the Chaudière Falls; and δίσκος, a round plate.

[2] But see footnote 1, *ante*, p. 547.



LEBETODISCUS.

IV.—Sedgwick Museum Notes.

A New Species of *Cyclus* from the Carboniferous Limestone of Ireland.

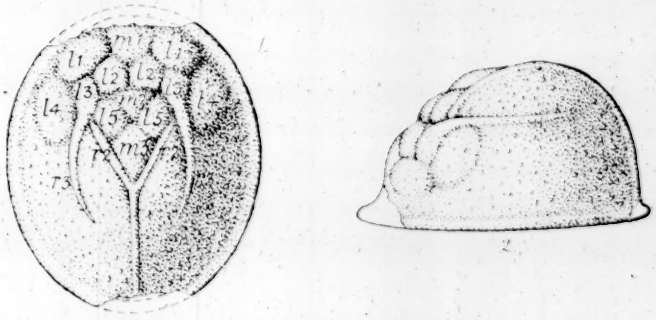
By F. R. Cowper Reed, M.A., F.G.S.

A SPECIMEN of *Cyclus* has lately been acquired from the Carboniferous Limestone of St. Doulaghs, co. Dublin, which indicates a new species, though it is allied to several which have been described by Dr. Henry Woodward. The latter[1] gave in 1894 a review of our knowledge of this curious little Crustacean genus, and his view that it is an Entomostracan and probably one of the Phyllopoda has been generally accepted, though more recently[2] (1905) he has suggested that the members of this genus are but the larval stages of Coal-measure Limuloids.

Our specimen is in a good state of preservation and is perfect, except round the anterior end, where the border is rather hidden by hard matrix, but all the bosses on this part of the carapace seem to be exposed.

Cyclus simulans, sp. nov.

Diagnosis.—Carapace oval in outline, convex, nearly hemispherical, greatest height somewhat behind middle, posteriorly descending more steeply to margin; surrounded by narrow border of regular width, horizontally extended, with rounded raised rim and shallow marginal furrow.



Cyclus simulans, sp. nov.
Fig. 1.—Top view. × 12.
Fig. 2.—Side view. × 12.

Anterior third of carapace elevated into regularly disposed contiguous bosses, and divided from hinder part by curved transverse cervical furrow. Four pairs of lateral bosses and one unpaired median one present. Innermost posterior pair (l_2, l_2) subquadrate, in contact in middle line and with median boss (m_1) in front and with cervical furrow behind. Outer posterior pair (l_3, l_3) subtriangular, lying along cervical furrow, wedged in between innermost posterior pair (l_2, l_2) and outermost posterior pair (l_4, l_4), and in contact with anterior pair (l_1)

[1] Woodward, Geol. Mag., 1894, Dec. IV, Vol. I, pp. 530–539.
[2] Ibid., 1905, Dec. V, Vol. II, pp. 490–492.

in front; prolonged backwards into curved lateral ridges (r_3, r_3) on slopes of hinder part of carapace. Anterior lateral pair (l_1, l_1) transversely oval, lying in front of l_3 and l_2, and touching median unpaired boss (m_1) and outermost posterior pair (l_4). Median anterior unpaired boss (m_1) small, subrhomboidal. Outermost posterior bosses (l_4, l_4) largest of all, obliquely placed, oval, touching in front the lateral bosses l_1 and l_3, and the curved lateral ridges above, surrounded by rather strongly marked furrow.

Posterior part of carapace divided in half by narrow median dorsal ridge (r_1) running forwards from posterior margin to middle of carapace, then bifurcating so as to enclose a triangular space (cardiac region) between its branches (r_2, r_2) and the cervical furrow, against which they end. Cardiac region triangular, occupied by a large rhomboidal posterior median boss (m_3) and a smaller anterior triangular boss (m_2), the latter flanked by a pair of subrhomboidal lateral bosses (l_2, l_2). Lateral ridges (r_3, r_3) proceeding from outer posterior bosses (l_4, l_4), curved, narrow, rounded, thinning and dying out at about two-thirds the length of the carapace without meeting dorsal ridge (r_1). Surface of carapace coarsely granular, with minute scattered puncta on lateral slopes, which become rather larger and more numerous near the marginal groove.

Length . . .	5·50 mm.
Width . . .	4·75 ,,
Height . . .	2·50 ,,

Affinities.—This species shows points of resemblance to several British forms already described. The bosses on the cardiac region and bifurcation of the dorsal ridge resemble *C. Harknessi*, Woodw.,[1] but the posterior and lateral parts of the carapace are quite different. In the dorsal ridge, lateral ridges, smooth sides, and border we are reminded of *C. Wrighti*, Woodw.,[2] but the bosses on the anterior and cardiac portions are differently developed. *C. bilobatus*, Woodw.,[3] is less closely allied. In *C. Scotti*, Woodw.,[4] the lateral ridges join the dorsal ridge behind, and the carapace is emarginate, but the ornamentation seems similar. The Irish species *C. Jonesianus*, Woodw.,[5] may also be compared.

V.—The Age of the 'Old or Grey Granite' of the Transvaal and Orange River Colony.

By Dr. C. G. S. Sandberg.

IT may be taken for granted that everybody agrees with Dr. Molengraaff when, in discussing Dr. Hatch's paper on "The Oldest Sedimentary Rocks of the Transvaal," he says:[6] "There must exist, or have existed,

[1] Woodward, Geol. Mag., 1870, Dec. I, Vol. VII, p. 556, Pl. XXIII, Figs. 6, 6*a*.
[2] Ibid., p. 555, Pl. XXIII, Figs. 5, 5*a*.
[3] Ibid., p. 554, Pl. XXIII, Figs. 3, 3*a*.
[4] Ibid., 1893, Dec. III, Vol. X, p. 28, woodcuts A and B; 1894, Dec. IV, Vol. I, p. 536.
[5] Ibid., 1870, Dec. I, Vol. VII, p. 557, Text-figures 1, 2 on p. 558; 1894, Dec. IV, Vol. I, p. 535, Pl. XV, Figs. 1*a*, 1*b*.
[6] G. A. F. Molengraaff: Proc. Geol. Soc. S. Africa, to accompany vol. vii of the Transactions, p. xxix.

a formation older than the Witwatersrand Beds, because even at the very base of the Hospital Hill series conglomerates are known, the pebbles of which cannot obviously have been derived from the Hospital Hill series by denudation." It has, therefore, I take it, not been the intention of various authors to prove this self-evident truth, but only to try and definitely settle the still open question whether this older rock is yet represented in the geological sequence of the country, and, if so, what strata should be identified as such. The relative age of the 'old granite' or 'grey granite' has been the base and the cornerstone of controversy on this subject, and it therefore becomes imperative to put on record and to continually keep in mind the only reliable mode of determination of the age of eruptive rocks which we yet possess. It would be out of place here to insist upon the origin of abyssal eruptive rocks of the granite family (Rosenbusch's Tiefengesteine).

The eternal cycle may be considered generally recognised to-day as answering to the formula: Eruptive rocks are in part or *in toto* the product of the transformation at some considerable depth and under the influence of heat, pressure, time, 'agents-minéralisateurs,' etc., of sedimentary strata, which in their turn originated from eruptive rocks by their disintegration and the transport, sorting, and redeposition of the disintegrated elements.

It is self-evident that in general the basal sedimentary strata are most exposed to this intra-telluric transformation, whilst on the other hand it generally requires the abyssal rocks to get at or near to the surface before their disintegration can take place. Eruptive rock would, therefore, always be restricted to the base of the geological sequence but for the action of mountain-folding forces, and of the corrosive digestion by the unconsolidated magma of the superposed strata, along lines of least resistance, progressing more or less intensely in different parts of the sedimentary envelope. Although consequently eruptive rock is, generally speaking, a direct product of such sedimentary rock, older than those indubitably recognisable as such and covering it, it has been universally agreed to assign such geological age to an eruptive rock which corresponds with the time of its consolidation. This geological date is fixed by the age of the youngest sedimentary deposit, traversed, injected, or altered by the non-consolidated magma; and as it has now been conclusively proved that the action of an internal magma on the overlying sedimentary strata is most erratic, so that the lower strata might seem *not* to have been affected at all in one place, whilst in its immediate vicinity conclusive evidence of its intense action on the same and even very much younger strata is abundant, we may not under any circumstance reverse the rule and deduct the relative age of sedimentary strata from their seeming to have been or not to have been affected by a given eruptive rock before its consolidation.

Strange as it may seem, this is what has deliberately been done with regard to the question of the basal sedimentary deposit in the Transvaal and South Africa.

It may be that D. Dorffel started this topsy-turvy way of reasoning. It is certain that Dr. F. H. Hatch and Mr. E. Jorissen, relying on the

result of Dr. G. S. Corstorphine's conclusions, dispute with each other the title of being its strongest advocate.

Dr. A. Schenck,[1] in 1888, first made a distinction between the Witwatersrand Beds[2] and the Swazi Beds, placing the latter at the bottom of the sedimentary series, evidently exclusively because of the then prevalent ideas which assigned all gneiss and crystalline schists, as a matter of course, to the Archæan system. Dr. G. A. F. Molengraaff,[3] who was naturally impressed by the great similarity both in the composition of the individual members and in the succession of the component strata of the Barberton (Swazi) and W.W.R. Beds,[2] who was struck by the perfect conformability between the Barberton Schists and the enclosed conglomerate formation so identical with the W.W.R. Beds,[2] who lastly found both unconformably covered by much younger and identical formations, naturally placed them in the same system and on the same geological horizon at the base of our geological sequence. His ruling was for some time generally accepted, until in 1903 Mr. Dorffel[4] reopened the question with a paper, the essence of which is contained in his final remarks—"At present I am not of opinion that the old granite is intrusive in the Witwatersrand Beds. . . . *If the old granite be not intrusive in the W.W.R. Beds, we have to assume the existence of an Archæan formation.*" (The italics are mine.) Dr. G. S. Corstorphine, taking up this line of argument, soon afterwards concludes as to the non-intrusiveness of the old granite in the W.W.R. Beds, and to the 'consequent' existence of an Archæan formation, on the following grounds:—[5]

"On *one* farm (east of Heidelberg, Uitkyk No. 97) Dr. Corstorphine finds the actual contact of the 'old granite' and the Lower W.W.R. Beds *along an exposure of* 200 *yards*. He cannot discover any intrusion of granite in the quartzites, nor any contact-phenomena between the two rocks. He furthermore cannot find any granite pebbles in the superposed quartzites. Rounded, water-worn boulders of white and bluish vein-quartz, which he is convinced originated from the breaking up of some of the numerous veins in the granite, are, however, conspicuous in these lower quartzites. He is impressed by the granite under the quartzites here showing a rounded and worn surface, and finally declares that he has not been able in any of the localities which he had the opportunity of examining elsewhere to recognise a series of schists forming the lowest portion of the W.W.R. series. What have been taken for schists and even for quartzites in several localities are really, according to Dr. Corstorphine, differentiations and variations in the granite itself."

It must at once strike the mind of the reader that Dr. Corstorphine

[1] A. Schenck, "Die Geologische Entwickelung Süd-Africas": Petermanns Mitteil., 1888, Bd. xxxiv, pp. 225–32.

[2] For Witwatersrand Beds we shall henceforth simply write W.W.R. Beds.

[3] G. A. F. Molengraaff: "Geologische Aufnahme der Süd-Afrikanischen Republik" (79 + xvii pp. in 4to, Pretoria, 1898), pp. 10, 27, and 35–8.

[4] D. Dorffel, "Note on the Geological Position of the Basement Granite": Trans. Geol. Soc. S. Africa, 1903, vol. vi, pt. v, pp. 104, 105.

[5] G. S. Corstorphine, "The Geological Relation of the Old Granite to the Witwatersrand Series": Trans. Geol. Soc. S. Africa, vol. vii, pt. i, pp. 9–12.

distinctly disowns the well-known most erratic manner in which igneous rock in its unconsolidated state affects adjacent sedimentary strata. Still, I am convinced that he is not ignorant of the fact that eruptive rocks send out intrusive offshoots, sometimes high into the covering sedimentary strata, right into very recent deposits at one place, whilst their action appears hardly perceptible even at their contact with the oldest covering rock at another place. It is also surely not unknown to him that such extreme effects may be situated in immediate vicinity to one another.

Lastly, may it now be taken as generally recognised that the intensity of the action of the unconsolidated eruptive magma on the sedimentary strata is more or less proportionate to the intensity of the effect of the mountain-folding energy which was brought to bear upon them, and stands in distinct relation to the position of the respective rocks in the tectonic structure thus engendered.[1]

That Dr. Corstorphine should generalise and conclude that the 'old granite' is not intrusive in the W.W.R. Beds, because over the vast 'old granite' area of the Transvaal, the Northern Orange River Colony, North-Eastern Cape Colony, and Southern Rhodesia he discovers one single place where over the extent of the exposed contact between this granite and the overlying W.W.R. Beds (200 yards) he is unable to find any evidence of contact-metamorphism, seems rather hazardous and a somewhat premature conclusion.

Neither the discovery in the W.W.R. quartzites of the rounded bluish quartz-pebbles, of extremely problematic origin, at the contact of the granite and these W.W.R. Beds, nor the determination of the sericite-schists occurring below the Orange Grove quartzites to be segregations of the granite (in spite of the absolute conformability of these schists to the lowest W.W.R. Beds and the presence of conglomerates composed of quartz-pebbles[2] in them), seems to render more satisfactory the conclusions arrived at. Nevertheless, they are eagerly seized upon by Dr. F. H. Hatch and Mr. E. Jorissen to serve as the basis of their arguments for 'proving' the existence of an Archæan formation older than and unconformable to the W.W.R. Beds.

Even supposing that it had already been demonstrated beyond the possibility of a doubt that in the Zoutpansberg district (and Barberton district, etc.) the granite is of the same age as the 'old granite' (of Vredefort, Johannesburg, and Heidelberg bosses), Dr. Hatch's reasoning must forcibly strike the unbiassed mind as singular.[3] He describes[4] the complex of highly altered crystalline schists at Mount

[1] C. G. S. Sandberg: "Etudes géologiques sur le Massif de la Pierre-à-voir (Bas Valais)," 129 pages in 8vo, Paris, 1905, pp. 108–10; "L'âge du granit des Alpes occidentales et l'origine des blocs exotiques cristallins des Klippes," C.R. Ac. Sc., 1905, t. cxl, pp. 1072, 1073 (10 avr.); "L'âge du granite Alpin," Arch. Sc. phys. nat. (4), Genève, 1907, t. xxiii, pp. 581–94. E. Weinschenk, "Grundzüge der Gesteinskunde," 2 vols., Freib.-i.-Breisgau, 1906.

[2] G. A. F. Molengraaff, discussion on Mr. Jorissen's paper, "Note on some Intrusive Granites, etc.," Proc. Geol. Soc. S. Africa, to accompany vol. vii, p. xxxii.

[3] Dr. G. A. F. Molengraaff: Discussion of above paper, Proc. Geol. Soc. S. Africa, to accompany vol. vii, pp. xxix–xxxi.

[4] Dr. F. H. Hatch, "The Oldest Sedimentary Rocks of the Transvaal": Trans. Geol. Soc. S. Africa, vol. vii, pt. iii, pp. 147–50.

Maré (Pietersburg), which, notwithstanding their altered state through contact-metamorphism, still surprisingly resembles, by his own admission, the Lower W.W.R. Beds. He cannot, either here or in the Barberton–Swaziland area, mention one single locality where to his own satisfaction undoubted W.W.R. Beds are found together with these highly metamorphosed rocks, and where they occur in a mutually unconformable relation. Everywhere the covering (if any) of these crystalline schists consists of strata considerably younger than even the Upper W.W.R. Beds. Still, he does not accept the probability of being in the presence of metamorphosed W.W.R. Beds, but jumps to the conclusion that these metamorphic rocks, so similar to the W.W.R. Beds, must be much older and belong to the Archæan system, simply because the 'old or grey granite' is supposed to be intrusive in these schists, and is 'proved' to be non-intrusive in the W.W.R. Beds!

Mr. Jorissen's reasoning by which he arrives at the same conclusion is stranger still, if possible.[1] He describes some eight different localities distributed over the whole of the Transvaal where he discovered the 'Archæan formation.' Like Dr. Hatch, he evidently cannot mention one single instance where he found W.W.R. Beds and the so-called older (Archæan) rock together. Like Dr. Corstorphine, he is satisfied that the sericite-schist separating the Orange Grove quartzites (and evidently conformable therewith, since he also does not mention the all-important fact of their being unconformable) from the granite on the farm Vergenoeg, No. 220,[2] is a segregation from the granite, and not due to the metamorphic action on quartzites of the yet unconsolidated granite (see above, p. 554). Further, on the farm Nooitgedacht, No. 565 (Pretoria District), Jorissen describes a patch of crystalline schists which he discovered in the 'old granite,' and by which it is transformed through contact-metamorphism.[3] This 'old granite' now occurs as a welcome oasis in the surrounding endless desert of typical new or red or Bushfeld granite right in the classical region from which the new granite derives its name, in the Bushfeld. It is clear that the only reason why Jorissen could assimilate the granite patch there with the 'old granite' is the presence of 'Archæan schists' evidently metamorphosed by it, whilst the only reason for concluding that he was in the presence of 'Archæan schists' here, must have been the fact of these schists having been transformed by contact-metamorphism with the 'old granite'![4] On the farm Malipskraal, No. 406 (Lydenburg), he describes some more 'Archæan rocks' which, according to Mr. Kynaston, are most likely a foliated norite belonging to the *Bushveld Series*.

Finally, Mr. Hall & Dr. Humphrey, of the Geological Survey of the Transvaal, accepting this topsy-turvy reasoning, assign certain

[1] E. Jorissen, "Notes on some Intrusive Granites in the Transvaal, the Orange River Colony, and in Swaziland": Trans. Geol. Soc. S. Africa, 1905, vol. viii, pt. iii, pp. 151–60; and discussion of Dr. G. A. F. Molengraaff on the above, Proc. Geol. Soc. S. Africa, to accompany vol. vii, pp. xxxi–xxxiii.

[2] E. Jorissen, loc. cit., p. 156.

[3] Loc. cit., p. 154.

[4] As a matter of fact, it is just possible that these schists, far from being Archæan, will prove to be nothing less than metamorphosed Pretoria series.

rocks of the Zwartkop,[1] which lithologically and in their mutual succession (only the order being reversed) are identical with Lower W.W.R. Beds, to this same Archæan system.

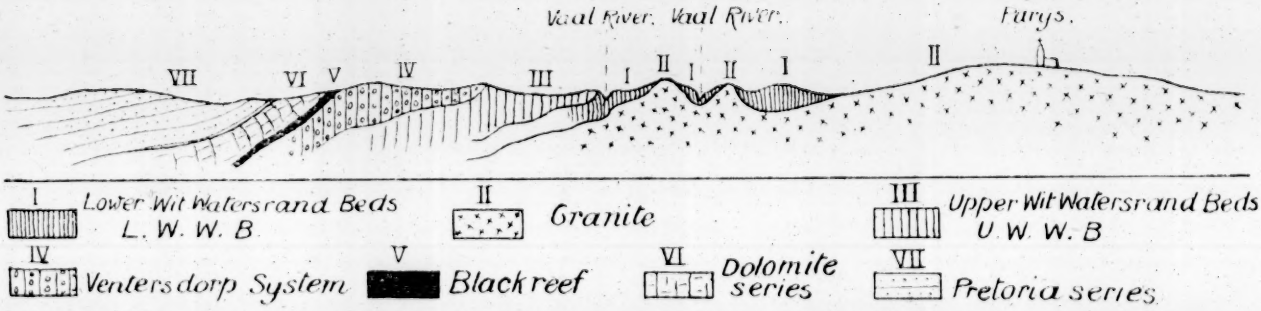
From the above it is clear that (*a*) no valid argument to prove the actual existence of pre-Lower W.W.R. Beds has yet been put forward; and (*b*) that even had Dr. Hatch's and Mr. Jorissen's conclusions been the logical outcome of their investigations, they must still be deemed unacceptable, being founded, as they are, upon the absolutely inadmissible basis of Dr. Corstorphine's conclusions. But if it is inadmissible to concede the non-intrusiveness of a granite mass from the fact that no injection nor any metamorphic action could be observed along *one* line of contact between that igneous rock and the overlying sedimentary strata, exposed over some 200 yards, it only needs the proof of the existence of *one single spot*, be it ever so small, where indubitable contact-metamorphism has taken place, to at once prove this igneous rock to be younger than the transformed sedimentary one. It is clear that the sericite-schists, separating over large areas in different regions the Lower W.W.R. quartzites from the underlying granite, are intimately connected with magmatic action, being the result either of magmatic segregation or of magmatic transformation of sedimentary rocks. And where now Molengraaff has been able to establish the presence in these sericite-schists of conglomerates (composed of quartz-pebbles), lying in absolute conformable position with and below the Orange Grove quartzites, there seems to be no more opening for doubting their sedimentary origin.

The conclusions arrived at from the study of these sericite-schists is materially strengthened by evidence from the Vredefort Massif. Certain strata of the Lower W.W.R. Series are here conspicuous by their peculiar and extensive development of corundum, which can only be attributed to the action on the overlying sedimentary strata of the yet unconsolidated magma.

But if, as evidence now already available seems to prove, the 'old granite' is post-W.W.R., where then have we to fix its approximate geological age? It is here where the great importance of the question lies. It is evident that the crystalline rock has participated in the orogenic movement, and that it has been folded together with the overlying W.W.R. Beds.[2] This fact finds its expression in the tangential direction to the granite mass periphery of the axes of the folds of the surrounding sedimentary strata, a phenomenon which is magnificently developed round the Vredefort granite mass. It is also evident from an examination of the contact region between this granite mass and the Lower W.W.R. Series, and of the small granite masses which come peeping through the overlying W.W.R. Beds on the farms Brakfontein (673), Koedoeslaagte (59), and Aasvogelrand (see Figure, p. 558). These granite protuberances, as well as the one situated further

[1] A. L. Hall & W. A. Humphrey, "The Blackreef Series and the underlying formation in the neighbourhood of Kromdrasi and Zwartkop, north of Krugersdorp": Trans. Geol. Soc. S. Africa, 1906, vol. ix, pp. 10–15.

[2] C. Sandberg, "Notes on the Structural Geology of South Africa": Trans. Inst. Min. Eng., vol. xxxiii, pt. v, pp. 540–57.



Ideal Section through the Vredefort Massif from Parys, W.N.W.

Basis, Dr. F. H. Hatch's Geological Map of Southern Transvaal.

north on the farm Rietfontein (No. 555), near the contact of the Black-reef and Ventersdorp series, most convincingly convey the impression of being situated on the anticlinal axis of the respective subordinate folds. They are consequently drawn out in the direction of the axes of these folds, and thus stretch in a curved line parallel to the semicircular periphery of the main granite mass.[1] Much further north and west we again find indubitable evidence, and now on a more gigantic scale, of the granite having been folded together with the overlying W.W.R. strata,[2] the Klerksdorp–Johannesburg granite masses being situated on the axes of the Pretoria–Blackreef anticlinal.

The Vredefort granite, as well as that of the Klerksdorp and Johannesburg (as also the Barberton and Zoutpansberg) regions (all supposed to be 'old or grey granite'), is, however, a homogeneous rock, hypidiomorphic in grain and massive in structure, showing no signs of crushing or contortion either macroscopically or microscopically. *It could therefore not have been folded after its consolidation.*[3] And as the W.W.R. Beds (at least during their last period of folding) have been folded together simultaneously with the younger deposits covering them, right up to and including the Pretoria Series, it would logically follow that the time of the consolidation, *that is, the age*, of the Vredefort, Johannesburg, and Klerksdorp granite masses must be fixed as *post-Pretorian*, that is, near to, perhaps even synchronous with, that of the so-called *Bushveld or new or red granite*. Since during the last couple of years the Bushveld area attracted considerable attention economically, our knowledge of the typical rock of the region was consequently considerably extended. It gradually became evident that all the features which once seemed to constitute characteristic differences between this red granite and the old or grey granite equally pertain to both rocks. There thus seems to exist a distinct consanguinity of the magmas. This and the great probability above demonstrated of these two igneous rocks being of approximately equal age, make it just possible that the old and the new or Bushveld granite are in reality identical, and derivatives from one and the same magma at the same period of 'eruption.' The numerous and powerful syenite dykes striking north and south, and evidently connecting the Johannesburg and even the Vredefort granite masses with the Bushveld igneous rock,[4] might then, perhaps, be explained as representing an endomorphic modification of this common magma due to the influence thereon of the overlying dolomite.

[1] On Dr. Hatch's geological map of the Southern Transvaal the phenomenon of the above-mentioned granite *fenêtres* is clearly expressed (Koedoeslaagte, No. 59).

[2] See also A. R. Sawyer, "New Rand Gold Fields, Orange River Colony": Trans. Inst. Min. Eng., vol. xxxiii, pt. v, pp. 530–4.

[3] E. Weinschenk: "Grundzüge der Gesteinskunde," i, pp. 168, 169. Since the assimilation of the Johannesburg–Vredefort–Klerksdorp 'old granite' with the Zoutpansberg–Swaziland–Rhodesia rock is exclusively based on lithological grounds only and must therefore be considered as inadequate, I have purposely, although personally inclined to regard these igneous rocks as identical, *not included* them directly in the summary of my argument.

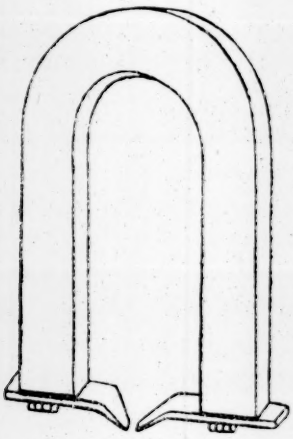
[4] A. L. Hall & W. A. Humphrey, loc. cit., p. 11, pl. iii.

VI.—A Simple Form of Permanent Magnet suitable for the Separation of Weakly Magnetic Minerals.

By T. Crook, F.G.S.

THE separation of the weakly magnetic minerals of crushed rocks, sands, etc., is best effected by using an electromagnet, which, if suitably constructed, is, for any given size, much stronger than a permanent magnet.[1] The electromagnet exerts no attractive force when the current is shut off, and for this reason the process of separation is made easy, as the grains which have been attracted to the poles drop when the current is switched off.

An ordinary permanent magnet of considerable strength has to be rather large; it requires re-magnetising at intervals; and to detach grains which have been attracted, it is necessary to strike them sharply with some object such as a suitably folded piece of paper or cardboard. Hence for speed, efficiency, and convenience in use, the electromagnet easily supersedes the permanent magnet. However, if an electric current is not available, as happens to be the case with the prospector, it becomes a decided advantage to have some handy form of permanent magnet capable of yielding an intense magnetic field. An electromagnet put into action by a hand dynamo has been tried, but such an apparatus makes a heavy load, and the dynamo readily becomes impaired. Indeed, under any conditions the use of a hand dynamo in making mineral separations is clumsy and inefficient, and involves unnecessary expense.



Permanent magnet with adjustable poles for securing a magnetic field of high but variable intensity.

It is therefore of interest to note that a simple permanent magnet of handy size can be made use of quite effectively in examining a small quantity of sand or crushed rock. It is convenient to have the magnet made U-shaped, the limbs being about 6 inches long. The

[1] For an account of the use of the electromagnet in this way see "The use of the Electromagnet in Petrography," *Science Progress*, No. 5, July, 1907.

bar of steel, of which the magnet is made, need not be more than an inch wide and half an inch thick. As shown in the accompanying figure, two adjustable pole pieces, made of soft iron, fit against the smooth free ends of the limbs, to which they are secured by binding screws. These pole pieces should be about half an inch wide and rather less than a quarter of an inch thick; they should be slotted so that the gap between the tips of the poles can be enlarged or diminished according to requirements. The adjacent portions of the pole pieces should be bent downwards, not perpendicularly, but as shown in the figure, narrowing gradually to a width of about three-eighths of an inch at the tips. The bent down portions of the poles should also thin gradually towards the tips. In this way a fairly strong magnetic field can be obtained between the tips of the adjustable poles.

Theoretically, a compound magnet should be more efficient than a simple magnet of the same size. In practice, however, one finds that there is no appreciable advantage in using a compound magnet, perhaps owing to the less perfect fitting of the adjustable poles. The use of a simple magnet is therefore recommended, as it is much cheaper and is more readily re-magnetised.

Using a magnet such as the one here described, a good separation of weakly magnetic minerals can be made. Magnetite, pyrrhotite, and highly magnetic hæmatite, if present, should first be separated by a small, weak magnet. Ilmenite, garnet, hornblende, augite, hypersthene, etc., can be extracted with the tips of the adjustable poles well apart; while by bringing the poles nearer together monazite can be extracted quite easily.

A magnet having approximately the specifications here given was made recently for Mr. A. E. Kitson, F.G.S., by Messrs. Baird & Tatlock, at the writer's suggestion. It worked quite satisfactorily, although the fittings were in some respects defective. It was found that, using a small trial specimen of ilmenite-monazite-zircon sand, the magnet extracted the ilmenite with the poles well apart; adjusting the poles with their tips nearer together, the monazite was completely extracted, and an almost perfect separation of the three constituents was thus made.

This simple permanent magnet is also a useful piece of apparatus for class demonstration. It is comparatively cheap, and a geological laboratory can be equipped with several of them without much expense; and certainly no course of laboratory work in petrology can be considered complete, that does not equip a student with a practical knowledge of the usefulness of the magnetic method of isolating and separating minerals.

VII.—Burning Cliffs.

TOWARDS the end of January last the inhabitants of Lyme Regis were somewhat alarmed by the announcement that a portion of the cliffs on the eastern side of the town, towards Black Ven, was "on fire"; a "Full Report of the Volcanic Eruption" was soon afterwards published in the *Bridport News* for January 24. It was then stated that on Sunday, January 19, "dense vapour appeared at

intervals to rise from a mound on the edge of the cliffs, about half-way between Lyme and Charmouth," and that the burning portion "consisted of a large fallen mass of the cliff which had some time since slipped away from the body of the cliff." Mr. A. C. G. Cameron, who was at the time resident at Lyme Regis, explained that the case was one of spontaneous combustion, due to the decomposition of iron pyrites and the consequent generation of heat sufficient to ignite the bituminous shales of the Lower Lias.

Iron pyrites and marcasite (rhombic iron pyrites) are found more especially in the Lias shales. They are most abundant in the shales of the Lower Lias. At Black Ven, near Lyme Regis, there is a 'Metal Bed,' and material derived from this and other layers was formerly collected during the winter months for the preparation of copperas (sulphate of iron), sulphuric acid, and sulphur. Examples of marcasite from Lyme Regis are sold to visitors as 'angels' wings.'

In August, 1751, spontaneous combustion occurred in the bituminous shales of the Lower Lias near Charmouth.[1] This took place among fallen masses of the strata, owing to the decomposition of pyrites. In 1890 similar combustion took place further east, and in the *Daily Graphic* of February 19 there appeared a picture of the 'eruption' of Golden Cap.

In September, 1826, spontaneous combustion took place in the Kimeridge Clay near the east extremity of Ringstead Bay, at Holworth Cliff, adjacent to the promontory of White Nore. This combustion continued until 1829, although the extent of the surface of the clay which was burnt did not exceed 50 feet square.

Buckland and De la Beche state that "within this space are many small fumaroles that exhale bituminous and sulphureous vapours, and some of which are lined with a thin sublimation of sulphur; much of the shale near the central parts has undergone a perfect fusion, and is converted to a cellular slag. In the parts adjacent to this ignited portion of the cliff where the effect of the fire has been less intense, the shale is simply baked and reduced to the condition of red tiles, like that on the shore near Portland Ferry."[2] The occurrence of the burnt shale at Portland Ferry indicates that there a similar combustion took place.

Some of the above details are taken from the Geological Survey Memoir on the Jurassic Rocks of Britain, vol. iii, p. 308, and vol. v, p. 331.

H. B. W.

VIII.—Burning Cliffs on the Banks of the Mackenzie River, etc.

As bearing upon the phenomenon of the spontaneous combustion of bituminous beds of coaly or carbonaceous matter of any geological age (charged with pyrites) *in situ*, the account given by Sir John Richardson, C.B., F.R.S., in his "Arctic Searching Expedition; a Journal of a Boat-voyage through Rupert's Land and the Arctic Sea," 1851, vol. i, may not be without interest in this connection.

He writes (p. 176): "On the Mackenzie a shaly formation makes

[1] See J. Stephens, "An Account of an uncommon Phenomenon in Dorsetshire": Phil. Trans., vol. lii, p. 119.

[2] Trans. Geol. Soc., ser. II, vol. iv, p. 23.

the chief part of the banks and also much of the undulated valleys between the elevated spurs. It is based on horizontal beds of limestone and in some places of sandstone. Covering the shaly beds, there exists in many places a deposit of sand, sometimes cohering as a friable sandstone. . . . The shale crumbles readily and often takes fire spontaneously, occasioning the ruin of the bank; it is only by the encroachments of the river carrying away the débris that the true structure is revealed."

"When exposed for even a short time to the atmosphere, the coal, which is probably all or mostly of Tertiary age, splits into rhomboidal fragments, which again separate into thin layers, so that it is difficult to preserve a piece large enough to show the woody structure in perfection. Much of it falls eventually into a coarse powder; and if exposed to the action of moist air in the mass it takes fire and burns with a fetid smell and little smoke or flame, leaving a brownish-red ash, not one-tenth of the original bulk of coal taken from the purer beds, for some contain much earthy matter." (p. 187.)

"From the readiness with which the coal takes fire spontaneously, the beds are destroyed as they become exposed to the atmosphere; and the bank is constantly crumbling down, so that it is only when the débris has been washed away by the river that good sections are exposed. The beds were on fire near Bear River when Sir Alexander Mackenzie discovered them in 1785, and the smoke, with flames visible by night, has been present in some part or other of the formation ever since." (p. 188.)

"Potter's clay, of a grey or brown colour, alternates with the beds already named, in layers varying from one foot to forty or more in thickness. This clay is often highly bituminous, and is penetrated by ramifications of carbonaceous matter, resembling the roots of vegetables. About 10 miles above Great Bear River, a layer of this material, lying immediately over a bed of coal which was on fire, has been baked so as to resemble a fine yellowish-coloured *biscuit porcelain*.[1] In a part of this I found numerous impressions of leaves, most of them Dicotyledonous, but one of them apparently coniferous and belonging probably to the yew genus." (p. 190.)

"Chief Factor Alexander Stewart told me that beds of coal are on fire on the Smoking River, which is a southern affluent of the Peace River, and crosses the 56th parallel of latitude, and also that others exist on the borders of Lesser Slave Lake, that lies between Smoking River and Edmonton. There are coal beds on fire, also, at the present time near Dunvegan on the main stream of the Peace River. All these places are near the base of the Rocky Mountains or the spurs issuing from that chain, and their altitude above the sea varies from 1,800 to 2,000 feet and upwards. The beds at Great Bear River are probably not above 250 feet above the sea-level." (p. 195.)

H. W.

[1] These porcellanous shales, with plant impressions of Tertiary Dicotyledonous leaves, etc., collected by Sir John Richardson on the cliff banks of the Mackenzie River from above the burning coal-seams, are now preserved in the Geological Department of the British Museum (Nat. Hist.), Cromwell Road. Many of the species agree with those from the plant-beds of Atanekurdluk, Greenland.

IX.—Additional Note on *Loricula*.

By H. Woodward, LL.D., F.R.S.

SINCE the appearance of my paper on *Loricula Darwini* in the November Number of this Magazine (pp. 491–9) my attention has been called by Mr. C. D. Sherborn to two American *Loriculæ* described by W. N. Logan, "On some new Cirriped Crustaceans from the Niobrara Cretaceous of Kansas," U.S.A., published in the Kansas University Quarterly (series A, October, 1897, vol. vi, No. iv, pp. 187–9, 8vo) and in the University Geological Survey of Kansas (vol. iv (Paleontology), pt. viii, 4to, Arthropoda, pp. 498–501, pl. cxi) under the names of *Stramentum haworthii*, Williston, type-specimen figured much enlarged, and *S. tabulatum*, Logan. The other two species on pl. cx, figs. 3–5, of the same work, described under the name of *Squama spissa* and *S. lata*, Logan, are quite distinct from *Loricula*, but are said to have been found adhering to a fragment of shell of *Inoceramus* by their entire length. The arrangement of the capitulum and peduncle differ very widely from those of *Loricula* and resemble *Pollicipes*.

The type of *Stramentum haworthi*, Williston, is said to be attached to a shell of *Ostrea congesta*, "by the extremity of its peduncle." (Possibly this remark applies to the genus *Squama*, as the description of the mode of attachment of *Squama* certainly applies to *Stramentum*.) The series of *Stramentum* preserved in the British Museum (Nat. Hist.) are certainly attached by their entire length upon the surface of some curious strap-like organism which might have been once a vegetable substance such as a *Laminaria*, but of which now only a stain remains on the slab. The specimens are very minute, only a few lines in length, but are illustrated by a copy of an enlarged figure of the type. The rostrum is certainly absent as in *Loricula* from the English and Bohemian Chalk, with which the type of *Loricula* (*Stramentum*) *haworthi* from the Yellow Chalk of Gove City, Gove County, Kansas, closely agrees. The other species mentioned, *Loricula* (*Stramentum*) *tabulatum*, is from the Upper Niobrara Chalk of the Smoky Hill River.

NOTICES OF MEMOIRS.

I.—Is China Clay a Mineral?

OF considerable interest to geologists is the judgment delivered by Mr. Justice Eve in the case of the Great Western Railway Company *v.* the Carpalla United Clay Company. The action was brought by the Railway Company to restrain the working of china clay in certain lands—the right to work the clay depending upon the question whether china clay was a mineral within the meaning of the Railway Clauses Consolidation Act, 1845. The following are the concluding portions of the judgment:—

This is the substance which the defendants contend is a mineral within the statute, and which the plaintiffs allege to be the soil or

subsoil which they have already purchased, and on which their railway has been built. In order to determine which of these contentions is right it is necessary to inquire into the composition and origin of the china clay. As I have already said, the china clay rock occurs in the granite formation only. Granite is an igneous rock the mineral constitution of which differs in various places, but which may be said to be practically of general uniform crystalline constitution composed of felspar, quartz, and mica. Granite when exposed to atmospheric or other agencies becomes decomposed to an extent varying with its mineral constitution, and the first element which is the subject of decomposition is the felspar. The decomposed felspar results in the formation of a clayey material, and china clay rock is granite in which the felspar has been wholly decomposed and replaced by this clay. Different opinions are held by geologists and others as to the agency by which this complete decomposition has been brought about. Some contend that the decomposing agent is the carbonic acid in the rain-water. Others, again, reject the rain-water, or subaerial theory, and insist that all the known facts combine to prove that the agency by which such complete decomposition has been brought about had its origin in subterranean depths, and that the agent penetrated to the decomposed mass by means of cracks and fissures, many of which are now filled up with minerals which admittedly came from lower depths. A third class of scientists, represented by some of the most eminent who have given evidence in this case, incline to the view that both causes—subaerial and subterranean, or pneumatolytic as the latter has been called—may have contributed to the result. Between these conflicting opinions it is fortunately not necessary for me to decide. It is sufficient for my purpose to find as a fact that there are in the granite formation in the part of the country with which I have to deal in this case nests or pockets of varying superficial areas, and in most instances of unknown depths, wherein is to be found a granite in which the felspars have been wholly decomposed and replaced with the clayey material I have already mentioned. It is further established by the evidence that these nests or pockets are sporadic, and that their existence adds materially to the value of the land, and that their presence or absence is not to be accounted for by any apparent differences in the overlying granite or other materials. As a general rule it may, I think, be said that they occur under an overburden of less decomposed granite, but, as I have already stated, they have been found under hard undecomposed granite, and under a wholly alien overburden such as the killas. Even when it occurs under an overburden of decomposing granite china clay rock has, I think, characteristics apart from colour which differentiate it sufficiently from the overburden to enable those acquainted with the local formation to fix approximately the line of demarcation between the overburden and the china clay rock. The decomposed felspar—the clayey substance which has replaced the felspar—constitutes, say the defendants, the china clay. All we do, they add, is to extract this clayey substance by a sifting or washing process, whereby we disengage it from the other material with which it is found in

mechanical combination. I cannot myself see that it is any answer to this contention to say that in the china clay of commerce there is still to be found a proportion of the other materials with which the clayey substance was originally completely combined. Take, for example, the presence of mica crystals in the commercial china clay. It is admittedly one of the objects of the washing process to separate the clayey substance from the mica, and this to a large extent is achieved, but because it is not wholly effected, and because the china clay is merchantable, notwithstanding the continued presence of some mica crystals, can it logically be asserted that mica is an essential constituent of china clay? I cannot bring myself to adopt any such view. In my opinion china clay or kaolin is the clayey substance in the china clay rock representing the decomposed felspar, and the mere fact that in the process of separating and extracting it from the rock a condition of disengagement is reached which is sufficient for practical commercial purposes, and beyond which it is therefore unnecessary to prolong the process, cannot, in my opinion, alter the real nature of the substance or convert the resultant product from a natural substance into an artificial combination of diverse elements originally combined in wholly different proportions.

On the evidence, therefore, I come to the conclusion that china clay is a natural product—that is, the substance representing felspar in granite which has been converted into china clay rock by the complete decomposition of one of its three essential constituents. The question I have now to decide is whether such a clay as I have described is a mineral. It is common ground that it has been so regarded by geologists, mineralogists, and textbook writers for very many years past, not only in this country, but in America, France, and Germany. Jameson, as early as in 1820, Professor Lapworth himself as late as in 1899, and Dr. Hatch and Professors Dana and Miers—the latter the well-known Professor at Oxford—at intermediate dates are all responsible for well-known and authoritative works, wherein it is classed as a mineral. It is true that when the witnesses for the railway company were confronted with these authorities they drew a distinction between kaolin and the china clay of commerce, and suggested that the former might possess attributes which would qualify it as a mineral, but which were not to be found in the latter; but I attach no importance to this distinction, in that I regard kaolin and china clay as convertible terms, and the mere fact that the clay can be turned to commercial uses without being altogether dissociated from foreign substances cannot, in my opinion, alter its real character. But the question does not really rest on the printed authorities to which I have just alluded. The scientific witnesses who were called on behalf of the railway company frankly admitted that down to some time in the latter part of last year they shared in the generally accepted view that kaolin or china clay was a mineral. Indeed, in a case tried in 1904—North British Railway Company *v.* Turners (Limited)—Professors Boyd Dawkins and Lapworth, two of the witnesses who in this case have been called to prove that it is not a mineral, gave evidence that kaolin or china clay—treating the two words as synonymous—is a mineral of a definite chemical composition,

and having very frequently a definite crystalline form. These views they have been led to discard, so they told us in the box, by more careful microscopic and local examinations of china clay, and the sources from which it is derived, and the conclusions to which these examinations have led them are directly opposed to those in which they shared with the scientific world generally down to the summer of last year. They now degrade china clay to an artificial product, a heterogeneous compound or mixture of everything that is in the china clay rock, and unredeemed by any one of the qualities which Dr. Hatch says are the essential characteristics of a mineral—that is to say, definite mineral composition, definite physical qualities, and definite crystal form. On the other hand, Professor Gregory, with whose evidence I was much impressed, would be no party to what I have called the degradation of kaolin or china clay. In his opinion it is a mineral, the main bulk of which is kaolinite—a crystalline substance which all parties agree is in all senses of the word a mineral. It is right that I should add that none of the plaintiffs' witnesses would admit the presence of kaolinite in the Carpalla kaolin, and on the evidence as it stands I should not be prepared to hold that this has been conclusively established. But again I am not really called upon to decide between the conflicting views of scientific men as to the exact category in which this china clay should be included to secure that accuracy of expression at which science is always aiming. What I have to determine is whether the substance is a mineral within the meaning of the Act of Parliament. Having heard all the evidence and listened to the forcible arguments which have been addressed to me, I cannot entertain any doubt as to its being such a mineral. It is found in intimate combination with elements which go to make up the subsoil of the district, and it owes its origin to the decomposition in past ages of constituent parts of that subsoil; but in its present condition, occurring sparsely and sporadically, and always under an overburden of a character distinctive from the rock in which it is found, it cannot, I think, with any justice be regarded as constituting the land soil. It is a sedentary deposit occupying the space formerly occupied by the felspars. It can only be abstracted by the disintegration of that wherein it is deposited, and when so abstracted it is a thing which (to use Mr. Justice Buckley's words, 1901, 2 Ch., at p. 638) "has a value of its own apart from the soil in which it is found." It is not, in my opinion, the soil itself. Whatever be the true scientific definition of a mineral, and whatever be the correct classification of kaolin thereunder, I cannot bring myself to hold that a substance universally regarded as a mineral before, and for more than sixty years after the passing of the Act which I am construing, ought now to be treated as not falling within the class of substances therein referred to as minerals. Under all the circumstances, therefore, I do not consider the facts of this case bring it within either of the authorities which have been so fully discussed, and holding as I do that the china clay is a mineral within the meaning of the Act of 1845, I have no alternative but to dismiss the action with costs.—Abstracted from the *Times*, 1908.

II.—Brief Notes on Nova Scotian Geology.

1.—A Review of the Flora of the Little River Group. By G. F. Matthew, D.Sc., etc. Trans. Roy. Soc. Can., Second Series, 1906, vol. xii, sec. iv, p. 99.

THIS is the first of a series of articles by Dr. Matthew on the ancient flora, the species of which were described by Sir W. J. Dawson many years ago. The present paper is devoted to the Equisetales, and a number of new forms are described in it. Two new genera are described, found in the lowermost group of plant beds—*Ramicalamus* and *Lepidocalamus*. Dr. Matthew finds *Calamites Suckovii* common, a species which is found in the uppermost beds of the Carboniferous, and therefore must have had a great vertical range. The genus *Asterocalamites* (*Calamites transitionis* of Dawson) is found to be common.

Asterophyllites and Annularia are represented in a number of species, including some in which the leaves are clustered in whorls, and so not evenly distributed. The early development of the Equisetales, as shown by this flora, is commented on in the closing paragraphs of this article.

2.—A New Species and a New Genus of Devonian Plants. By G. F. Matthew, F.R.S.C. Bull. Nat. Hist. Soc. of New Brunswick, 1906, vol. v, pt. iv, p. 393.

THE plant here described is referred to the Ferns, and thought to be related to *Eremopteris* and *Triphyllopteris*. The leaves of the barren frond were narrowly wedge-shaped, and there was a fertile pinnule with pod-like receptacles. The name adopted for this plant is *Pseudobaiera McIntoshi*. With this occurs a vigorous-growing mutation of *Annularia longifolia*, Brong., and other species of plants. The locality is near St. John, N.B., Canada.

3.—On some Species of Silurian and Devonian Plants. By G. F. Matthew, LL.D., etc. Trans. Roy. Soc. Can., Third Series, vol. i, sec. iv, p. 185.

A FEW Acrogens from various localities in the provinces of New Brunswick and Nova Scotia in Canada are described in this paper. A cone of Lepidodendron of Eo-Devonian; a supposed lichen of the same age or older, and plants from the Upper Devonian of Nova Scotia and N.B. Two interesting fungi which grew between the wood and epidermis of certain large ferns are described, and also a new species of *Psilophyton* which may have been of greater antiquity. *Lepidodendron corrugatum* and *Aneimites Acadica* are common forms of the Upper Devonian flora.

4.—Notes on Archæozoon. By G. F. Matthew, LL.D., F.R.S.C. Bull. Nat. Hist. Soc. of New Brunswick, 1906, vol. v, p. 547.

A LOW type of calcareous organism found in the pre-Cambrian rocks of St. John, N.B., Canada, and thought to be related to the Cryptozoon found in the pre-Cambrian deposits of the Rocky

Mountains by officers of the United States Geological Survey. It consists of calcareous columns with convex layers. Three localities are known where it has been found, and in a plate accompanying the paper its structural characters are exhibited.

5.—A New Genus and a New Species of Silurian Fish. By G. F. Matthew, LL.D., etc. Trans. Roy. Soc. Can., Third Series, vol. i, sec. iv, p. 7.

THIS is a description of an ancient and interesting type of fish of early Silurian age. The form is related to *Phaneropleuron*, from which it differs in the arrangement of the fins, etc. It is from strata in King's County, New Brunswick, Canada.

6.—The Physical Evolution of Acadia. Part I: The Insular Stage, etc. By G. F. Matthew. Bull. Nat. Hist. Soc. of New Brunswick, 1907, vol. vi, p. 3.

IN this article the bearing of the geological changes which occurred in the maritime provinces of Canada prior to Devonian time is shown. The history is divided into several periods by the physical revolutions that occurred. The first period (called the Laurentian phase) is marked by the occurrence especially in Southern New Brunswick and in Cape Breton of abundant limestones, which are compared to the Granville limestones of the Ottawa valley.

The second great period (Huronian phase) is marked in Nova Scotia by the enormous deposits of the gold-bearing series of that province, and is compared to clay slates, chloritic slates, and other rocks in Southern New Brunswick, which are thought to be a deep-water representation of the gold-bearing series of Nova Scotia.

The third great period of deposition of sediments is marked by the widespread Cambrian deposits of the Atlantic region of Canada, which are best shown in Southern New Brunswick and Cape Breton Island in Nova Scotia. This great series in both provinces runs up into and includes the lower part of the Ordovician.

This series is followed by an important geological hiatus, the upper Ordovician being absent from all this region, and the first rocks which succeed the Cambrian series are of the age of the Ludlow or thereabouts. This is the Silurian phase, and extends upward to include the base of the Devonian (if the Upper Helderburg formation be regarded as such).

Up to this time in its geological history Acadia when submerged in part was insular, or divided from the rest of the American continent by one or more sounds, extending from the present Gulf of St. Lawrence to the Gulf of Maine, and further inland in New England. Southern Acadia up to this time was dominated by insular conditions, and for most of the time prior to the Devonian age was an island cut off from the mainland of America.

Two maps are given to illustrate this history, one showing the conditions in the middle Lower Huronian time, and the other drawn to show its aspect in Upper Silurian time (Clinton to Niagara).

REVIEWS.

I.—The Face of the Earth. By Edward Suess. Translated by Hertha B. C. Sollas, Ph.D.; under the direction of Professor W. J. Sollas, LL.D., F.R.S. Vol. III. 8vo; pp. vii, 400, with one map (in pocket), 6 plates, and 23 other illustrations. Oxford: at the Clarendon Press, 1908. Price 18*s.* net.

ATTENTION has been drawn in the Geological Magazine (for May, 1905, and July, 1906) to the first and second volumes of the English edition of Suess's great work. The present volume constitutes part 4, or vol. iii. part 2, of the original German edition. It is divided into seven chapters, and Professor Sollas, who has continued his labours as editor, has been assisted in the revision of the translator's rendering by Sir Archibald Geikie, Dr. Teall, Professor Edgeworth David, Professor Watts, Mr. R. D. Oldham, Professor T. C. Chamberlin, Professor Lapworth, and Professor Bonney. We give the names in sequence according to the chapters revised by them; but Professor Sollas remarks in his preface, "The reverence due to a great classic has restrained us in this, as in previous volumes, from taking any liberties with the text, whether by comment or emendation."

The footnote references have been extended so as to include researches published during the present century, but we miss a reference to the Geological Survey Memoir on "The Geological Structure of the North-West Highlands of Scotland," 1907, which might have been given on p. 387. Britain, however, occupies but little space in the present volume, and only in connection with the Caledonian lines of disturbance.

An interesting outline of the contents is given in the introductory chapter, where we find remarks on the folding of the oldest rocks and the trend of the systems of folds which form the mountain chains; on the distinction between longitudinal volcanic lines and independent volcanic lines, and the impossibility of considering folds and volcanoes apart from one another; and on the need of studying the structure of mountain chains in plan as well as in section.

In reference to the subjects under discussion, it is remarked that "In the Southern Hemisphere the space covered by the sea is so great, and our knowledge so incomplete, that we can scarcely expect to arrive at any important conclusions. The present investigation is therefore almost exclusively confined to the Northern Hemisphere, and more especially to that region which lies north of the southern boundary of Eurasia and of the Caribbean Sea." The map appended to the volume does not include the entire area. It is a "Diagrammatic Representation of the Vertex of Eurasia," and it takes in the country between Lakes Balkhash and Baikal, the Thian-Shan, and the upper regions of the Hoang-ho. The region around Lake Baikal is described as "the most ancient vertex of the Eurasian folds."

In the Introduction it is pointed out that "all the Archæan rocks of the earth have suffered folding or an equivalent compression," and that "folded ranges had been already levelled down in times preceding

the Cambrian.' When these worn-down ranges are covered by undisturbed Cambrian strata, as in the vicinity of St. Petersburg, we are compelled to conclude that the ancient folding has not been continued or renewed since an extremely remote period. In these regions, as compared with those where recent sediments are involved in the folding, it would seem that the earth is slumbering or as if the folding force had become extinct."

Reference is also made to areas where the mainland is accompanied by trend-lines of folding, and to others where this is not the case.

The main body of the work is taken up with the great structural features of Northern and Central Asia and Europe, and with the evidence in certain regions of repeated folding or reconstruction. The Asiatic folding is shown to be connected through the Caucasus with that of Europe, while the system of movements between the Arctic Ocean and the Mediterranean is not to be separated from that of Eastern Eurasia.

II.—The Philippine Islands.

The Mineral Resources of the Philippine Islands, with a Statement of the Production of Commercial Mineral Products during the year 1907. Issued by Warren D. Smith, Department of the Interior, The Bureau of Science. 8vo; pp. 39, illustrations and maps. Manila, 1908.

THIS is of necessity a preliminary account of work done, and it will be sufficient to call attention to the contents which follow:—The Non-metallic Minerals, by W. D. Smith; Metallic Mineral Resources, by Maurice Goodman; Statistics, by W. D. Smith; Mining and Geological Notes on a portion of North-Western Mindanao, by H. M. Ickis; Mining Prospects on and near the Zamboanga Peninsula (brief summary of the geology), by W. D. Smith; Summary of the chief characteristics of Philippine Ores, by W. D. Smith; Summary of the chief characteristics of Philippine Coals, by A. J. Cox. One of the maps shows the principal mineral districts of the country. We hope soon to possess a sketch-map of the geology of the whole group, which should prove of especial interest with regard to Japan on the one hand and the Javan-Bornean group on the other.

III.—South African Palæontology.

The Invertebrate Fauna and Palæontological Relations of the Uitenhage Series. By F. L. Kitchin, M.A., Ph.D. Ann. South African Museum, vol. vii, pt. ii, pp. 250, 10 plates. 8vo. Cape Town (West, Newman, & Co., London), 1908. Price 12*s.* 6*d.*

A PORTION of this volume is devoted to descriptions of fossils collected by the members of the Geological Survey of Cape Colony and others. We hope the author has sent a copy of his work to the Stationery Office in London, in order that they may see how Cape Colony produces its prints and plates, which are quite in accord with the importance of the subject.

The collection here described was obtained mostly by Messrs. Rogers

and Schwarz in 1900, but the author has availed himself of all other material on which he could lay his hands. Discussing first the age of the fauna, Dr. Kitchin has provided a careful summary of previous work in fourteen pages ranging from Hausmann's researches in 1837 to his own. He arrives at the conclusion that "no portion of the Uitenhage Series represents a period of time earlier or later than the Neocomian. It must be said, indeed, that the almost entire restriction of *Holcostephanus*, sensu stricto (= *Astieria* auctorum), to the upper part of the Valanginian and lower beds of the Hauterivian in Europe suggests much narrower limits, when we consider how important a place is taken by members of this genus in characterising the cephalopod-fauna of the Uitenhage beds."

The fauna is then compared with that of the south-west of Madagascar, the Oomia group in Cutch, certain Godavari and Hazara fossils, the Neocomian fauna of German East Africa, and the Neocomian fauna of Patagonia, and the author concludes his general remarks with a discussion on the distribution of the Uitenhage fauna in relation to some theoretical questions. These are the theory of an Indo-African land-barrier during early Cretaceous times and Neumayr's theory of distribution according to climatic zones. Then follows a description of the fauna and a bibliography.

The memoir, with its beautiful plates, cannot fail to be of the highest value, and marks a great advance in our knowledge of the fossil fauna of South Africa.

IV.—New Zealand.

THE most interesting paper in the Transactions of the New Zealand Institute, 1908, vol. xl, is Dr. Marshall's geology of the centre and north of North Island with its accompanying map. It seems that there is little evidence in regard to the structural meaning of the direction of the North of Auckland Peninsula. The plutonic rocks of Mangonui and Ahipara are diorites and norites, but no evidence is available as to whether they are intrusive or older than the Mesozoic sediments, and that the volcanic rocks are chiefly rhyolitic in the central region, but the rhyolites are penetrated by andesitic pipes, over which large cones have been built up. The lake-basins are probably areas of violent hydrothermal explosions, and from these explosions pumice was distributed. The sharp scarps of many of the rhyolite hills do not indicate the action of faults, but are due to erosion, and a sequence of eruptive rocks is suggested. J. A. Thomson writes on Tertiary fossils from Kakanui and R. Speight on some aspects of the Terrace-development in the valleys of the Canterbury Rivers. The Schists of Central Otago are described by A. M. Finlayson, who also writes on the Scheelite of the same district. The Westland Alkaline and Nepheline rocks are worked out by J. P. Smith, and a soda-amphibolite-Trachyte from Cass' Peak, Banks Peninsula, is described by R. Speight. The Gabbro of the Dun Mountain receives attention from Dr. Marshall.

V.—Notes on Fenno-Scandia.

J. J. Sederholm. Explanatory Notes to accompany a Geological Sketch-map of Fenno-Scandia. 8vo; pp. 31, map. Helsingfors, 1908.

THIS is one of those useful and comprehensive sketches of the geology of a large district so valuable to the busy geologist. The area included is the whole of Norway, Sweden, and Finland, and parts of Denmark, the North of Prussia, and North-West Russia. The map is 1 : 8,000,000, and is clearly printed in colour. The author points out that the material is of different value for different parts of the region, as large tracts are still untrodden by the feet of any geologist, but he has drawn on the publications of many colleagues, and produced a valuable compilation showing our present knowledge of this important northern European area.

REPORTS AND PROCEEDINGS.

Geological Society of London.

November 4, 1908.—Professor W. J. Sollas, LL.D., Sc.D., F.R.S., President, in the Chair.

On the Admission of Women as Fellows.

The President announced that the result of the communication addressed to the Foreign and Colonial Fellows of the Society with regard to the admission of women was as follows:—

Papers sent out	313
Answers received	124

Analysis of Replies.

1. Are you in favour of the Admission of Women to the Geological Society of London?

Yes	97	
No	27	
		— 124

2. Are you in favour of the Admission of Women as Fellows, or as Associates only? The 97 in favour of admission voted—

As Fellows	70	
As Associates	25	
Not specified	2	
		— 97

3. If there should not be a majority of those voting in favour of Women as Fellows, are you in favour of their Admission as Associates?

Yes	93	
No	3	
Not specified	1	
		— 97

The Secretary read a note received from the Under Secretary of State for the Colonies, embodying extracts from the Report on a Scientific Expedition to the Falkland Islands (October, 1907–February, 1908) by Dr. Carl Skottsberg. It was stated therein that the Devonian formation, which constitutes the larger part of the islands, was closely surveyed, and fossils were discovered in

several new localities. Also that new discoveries of fossil plants (*Glossopteris*, etc.) had proved that the whole southern part of the East Falkland Island, south of Wickham Heights, belongs to the Gondwana System.

The following communications were read:—

1. "The Relations of the Nubian Sandstone and the Crystalline Rocks of Egypt." By Hugh John Llewellyn Beadnell, Assoc. Inst. M.M., F.G.S. (late of the Geological Survey of Egypt).

The paper opens with an account of the general conclusions of previous observers, which are mainly in favour of the view that the granites are not intrusive into the Nubian Sandstone, but that the latter was deposited round denuded masses of the granite. The crystalline rocks, south of the Oasis of Kharga, are first dealt with. Eight exposures of crystalline rocks were met with. The sediments near the contact with the crystalline rocks are generally inclined at a high angle, and in some cases the former appear to undergo considerable alteration. The bedded rocks contain no fragments derived from the crystalline rocks. Hills of folded Eocene and Cretaceous strata seem to indicate that the intrusion of the granite may be of later date than Lower Eocene. These crystalline rocks do not appear to differ from those of the First Cataract and of the Eastern Desert and Sinai, where great vertical displacements have occurred, and where it seems likely that the sandstones were carried up when the great igneous core was elevated into its present position. Here too there seems to be no evidence of fragments of the crystalline rocks in question in the sediments. Thus the author concludes that the Nubian Sandstone was unconformably deposited, partly on pre-existing sedimentary formations, and partly on the planed-down surfaces of still older crystalline and metamorphic rocks. Subsequently it was invaded by outbursts from the underlying magma, the intrusions being probably connected with the elevation of the mountainous regions on the east side of the Nile.

2. "On the Fossil Plants of the Waldershare and Fredville Series of the Kent Coalfield." By E. A. Newell Arber, M.A., F.L.S., F.G.S.

At the boring at Shakespeare Cliff, Dover, Coal-measures were reached in 1890 at a depth of 1,100 feet, and subsequently penetrated to a depth of about 2,270 feet. Thirteen seams of coal, varying in thickness from 1 to 4 feet, were pierced. Coal-measures were struck at 1,394 feet at the boring in Waldershare Park, and pierced for 1,260 feet more. Five seams of coal, varying from 1 ft. 4 in. to 5 ft. 2 in. in thickness, were struck. The boring near Fredville Park reached Coal-measures at 1,363 feet, pierced three seams of coal, and was continued to a depth of 1,813 feet. The specimens of plants collected from the Waldershare and Fredville borings are dealt with in detail, and compared with plants found at Dover and in other localities in Britain and abroad. The more abundant and characteristic species are common to Waldershare and Fredville, and lead to the conclusion that the beds belong to the same horizon. The majority of species tabulated are either confined to the Upper Coal-measures and the Transition Series below, or are Middle and Lower Coal-measure forms which are known to occur in the Transition

Series. Indeed, all but two plants have been recorded from the last horizon. Thus the beds are the homotaxial equivalents of the Newcastle, Etruria, and Black Band horizons of North Staffordshire, the Hamstead Beds below 1,233 feet in South Staffordshire, the Coed-yr-allt Beds and Ruabon Marls of Denbighshire, the Ardwick Series and Beds above the Bradford Four Foot Coal in South Lancashire, the Lower Pennant Grit of South Wales, and the New Rock and Vobster Series of Somerset. The data with regard to Dover are too scanty for certainty, but they seem to indicate approximately the same horizon as the two other Kentish localities. The majority of species are also common to the highest zone, or the "Charbons Gras," in the Pas de Calais. The flora of these rocks, and of those on the same tectonic line, belongs to the lower of the two great Continental zones of the Upper Carboniferous—the Westphalian; and the higher zone, the Stephanian, is unrepresented in the Mendip-Artois series of basins. But, as this axis is followed from east to west, it appears that continuously higher horizons are met with.

CORRESPONDENCE.

HIPPONYX FROM THE WHITE CHALK.

Sir,—It may be of interest to note that a third specimen of *Hipponyx blackmorei* turned up almost immediately after the publication of my note in the Geological Magazine for October. This was recognised by Dr. Rowe in the collection of Mr. J. R. Farmery, of Louth, who, with other of our friends, has been patiently working out the Chalk fauna of Lincolnshire. The specimen came from the *Holaster planus*-zone of Boswell; it is affixed to a specimen of *Micraster præcursor*, is slightly better preserved than the type, and has an oval form, thus showing a characteristic variation of growth. The age of this specimen is of especial interest. Mr. Farmery has generously given this rare fossil to the British Museum.

C. Davies Sherborn.

FOREIGN FLINTS IN THE EAST COAST DRIFTS.

Sir,—Referring to Mr. Bullerwell's note on the number of flints in the old gravel-bed on the Northumberland coast, which appeared in the Geological Magazine for November (p. 525), I can endorse what he says as to the probable existence in the bed of the North Sea of chalk deposits. In our Holderness drifts we find quite a large number of masses of black flint and pink flint, both of which are different from anything occurring in this county. Formerly their presence was easily accounted for in the drift as being derived from Denmark. A Danish geologist, however, informed us that there is no flint in Denmark. In addition to the flint we obtain scores of chalk fossils, from a different horizon, however, from anything that occurs *in situ* in the county. These include, principally, the flint casts of a small sea-urchin, resembling *Ananchytes ovatus* in general shape, and some

well-preserved belemnites of the *lanceolatus* type, both these fossils occurring literally in hundreds in the drift. We have recently obtained two specimens of black flint in which these particular belemnites are embedded, and as the sea-urchins are usually in black flint, it would seem that all three are derived from an outcrop somewhere in the North Sea. In our lowest drifts, that is those which were deposited by the first advance of the glacier, are a number of green-coated black flints similar to those occurring in the Eocene deposits. These had been probably lying on the floor of the sea a considerable time before being taken up by the glacier.

T. Sheppard, F.G.S.

Hull.

OBITUARY.

WILLIAM JEROME HARRISON, F.G.S.

Born 1845. Died June 6, 1908.

We regret to record the death of W. J. Harrison, who did much to advance the progress of geological knowledge as an enthusiastic teacher and local worker in the neighbourhoods of Leicester and Birmingham, and also by means of bibliographic research.

Born at Hemsworth, near Doncaster, he early qualified as a science teacher, and ultimately wrote a number of elementary textbooks on natural science, chemistry, and physics. A fifth edition of his useful textbook of geology was issued in 1903, and he was author also of "Geology of the Counties of England and of North and South Wales," 1882. These works, as was the case with all his publications, were characterized by great care and accuracy.

For some years Mr. Harrison was Curator of the Leicester Town Museum. During this period, in 1874, he drew attention to his discovery of the Rhætic beds near Leicester, and in 1877 he issued "A Sketch of the Geology of Leicestershire and Rutland," reprinted from White's "History" and supplemented by twelve photographs.

In 1880 he removed to Birmingham, where he was appointed Chief Science Master under the Birmingham School Board. Here he devoted much attention to the Drift deposits, and published important bibliographies of Midland and Norfolk Glaciology, with brief notes of the contents of the papers, likewise a very full bibliography of Stonehenge and Avebury.

[A brief memoir of W. J. Harrison, with portrait and bibliography, to which we are much indebted, has been published in the *Naturalist* for September, 1908, by Mr. T. Sheppard.] H. B. W.

MISCELLANEOUS.

With the approval of H.M. the King, a Royal Medal has been awarded by the Council of the Royal Society to Professor John Milne, F.R.S., F.G.S., for his researches and investigations in Earthquake Phenomena in this country and in Japan, during more than thirty years.

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